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ABSTRACT

The colloquium examined the strengths and limitations of budget planning and formulation for R&D in the Executive and Legislative Branches of the federal government. Discussions and prepared remarks were presented on: (1) perspectives on the 1977 R&D budget; (2) problems in R&D budgeting; (3) budgeting for basic research; (4) budgeting for development and demonstration; (5) R&D budgets and technological advance; (6) R&D budgets and long-range planning; and (7) the impact of the congressional budget process on federal R&D spending. (BB)

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Research & Development in the Federal Budget

Colloquium Proceedings

June 15–16, 1976

Edited by
William A. Blanpied
Catherine Lighthizer

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Acknowledgments

The editors wish to express their appreciation to all who offered advice and suggestions during the preparation of these proceedings. Special thanks are due Roger Long of the AAAS Division of Public Sector Programs for yeoman's service.

Introduction

The 15-16 June Colloquium on Research and Development in the Federal Budget, arranged as an integral part of a pilot AAAS R&D budget analysis project, provided a forum for exploring several of the long-range and recurring issues identified in Willis H. Shapley's *Research and Development in the Federal Budget: FY 1977*. The colloquium was organized under the auspices of the AAAS Committee on Science and Public Policy, with guidance from the AAAS Board of Directors. Critical inputs to the colloquium were received from representatives of several scientific and engineering societies affiliated with the AAAS.

The objectives of the colloquium and, indeed, of the pilot budget analysis project itself were to examine critically the strengths and limitations of budget planning and formulation for R&D in the Executive and Legislative Branches of the federal government, to make the functions and substance of the budget and the budget process more comprehensible to the scientific community and to students of science policy, and to provide some initial feedback on budget-related issues to federal policy-makers from the scientific community. Additionally, the colloquium suggested appropriate dimensions for succeeding AAAS R&D budget reports, and served to test the feasibility of holding an annual, mid-year, AAAS science and public policy meeting in Washington, D.C.

Almost two-hundred participants from academia, private industry, state governments, and the Executive and Legislative Branches of the federal government attended all or part of the day-and-a-half colloquium which included: an opening plenary session featuring prepared addresses by representatives of the Executive Branch, the Congress, and the scientific community; four concurrent, half-day workshop sessions; and a closing plenary session featuring an address on R&D in the new congressional budget process by the Honorable Brock Adams. The sessions were invariably lively and frequently controversial. Whereas no agreement was either sought or reached on the specific substantive issues that were discussed, most of the participants emerged from the colloquium with a heightened appreciation of the complexity of budget-related issues. A remarkable degree of consensus about the desirability for the scientific community to play a greater role in the budget-making process was also evident, although it was agreed that, in order to play such a role, the community would have to become more attuned to the realities of science policy formulation.

The generally positive responses which the Shapley report and the June colloquium engendered have encouraged the AAAS to issue an annual series of R&D budget reports that will explore specific problem areas of each year's budget in greater depth than was possible in the preparation of the FY 1977 report, given the limited time and resources that were available. These reports will be capable of standing alone, and can also serve as starting points for discussions such as those that occurred at the June 1976 colloquium. It is the hope of the AAAS that the continuing budget analysis project will be instrumental in identifying common ground on which significant aspects of the relationship between science and government can be explored.

September
1976

William A. Blanpied, Head
Division of Public Sector Programs
AAAS

Agenda

Tuesday, 15 June 1976

12:00 noon Registration and Buffet Lunch
(Reception Room, Carnegie Institution)

2:00 pm **Opening Plenary Session**
(Auditorium, Carnegie Institution)
Presiding: William D. McElroy, *Chancellor, University of California, San Diego, and President, American Association for the Advancement of Science*
Speaker: Willis H. Shapley, *Consultant to NASA and the AAAS Committee on Science and Public Policy*
Topic: "Perspectives on the 1977 R&D Report"

2:30– 4:00 pm **Problems in R&D Budgeting**
Speakers:
H. Guyford Stever, *Director, National Science Foundation*
Paul O'Neill, *Deputy Director, Office of Management and Budget*
James W. Symington (D.–Md.), *Chairman, Subcommittee on Science, Research & Technology, House Committee on Science and Technology*
Paul Doty, *Director, Program for Science and International Affairs, Harvard University*

4:00– 5:00 pm **General Discussion**

5:00– 5:30 pm **Summary:** Ruth M. Davis, *Director, Institute for Computer Sciences and Technology, National Bureau of Standards*

5:30 pm Cocktail Reception
(Reception Room, Carnegie Institution)

Wednesday, 16 June 1976

Morning **Concurrent Workshop Sessions**

1. **Budgeting for Basic Research**
(South Scott, Gramercy Inn)

Presiding: Jurgen Schmandt,
Professor of Public Affairs, LBJ School of Public Affairs, University of Texas

Rapporteur: Pamela Ebert,
Executive Secretary, National Research Council Study on National Research Awards

Criteria for levels of support—The question of relevance—Securing and maintaining support—Dealing with inflation—Problems of assessment—Prospects for the future.

9:00– 9:10 am Introductory Remarks by Presider

9:10–10:00 am **Prepared Presentations:**

Philip M. Smith, *Special Assistant to the Director, National Science Foundation*

Ronald W. Lamont-Havers, M.D., *Deputy Director, National Institutes of Health*

Daniel R. Miller, *Deputy Director, Division of Physical Research, Energy Research and Development Administration*

10:00–10:30 am **Invited Comments:**

William G. Wells, *Technical Consultant, House Committee on Science and Technology*

Ronald M. Konkel, *Chief, Science and Space Programs Branch, Office of Management and Budget*

John Allen, *Deputy Director, Research and Advanced Technology, Department of Defense*

Clifford Grobstein, *Professor of Biology and Vice Chancellor, University Relations, University of California, San Diego*

N. Richard Werthamer, *Chairman, New York State Energy Research and Development Authority*

10:30–11:30 am **General Discussion**

11:30–11:40 am **Summary:** Rapporteur

11:40–11:50 am **Wrap-up:** Presider

2. Budgeting for Development and Demonstration

(North Scott, Gramercy Inn)

Presiding: Don E. Kash, *Director, Science and Public Program, University of Oklahoma*

Rapporteur: Christopher L. Coccio, ASME *Congressional Engineering Fellow with the Senate Committee on Interior and Insular Affairs*

Alternative agency approaches to management and budgeting for development—Relation to technology base—Determining the federal role in demonstration programs—Cost sharing with industry—Alternatives to direct federal support.

9:00– 9:10 am Introductory Remarks by Presider

9:10–10:00 am **Prepared Presentations:**

Ernest S. Chaput, *Director, Budget Programs, Office of the Comptroller, Energy Research and Development Administration*

G. R. Makepeace, *Assistant Director, Defense Research & Engineering, DOD*

Thomas Glennan, *National Academy of Sciences*

10:00–10:30 am **Invited Comments:**

Daniel Dreyfus, *Deputy Staff Director for Legislation, Senate Committee on Interior and Insular Affairs*

Jon Veigel, *Director, Research and Development, California State Energy Resources Conservation and Development Commission*

Walter Baer, *The RAND Corporation*

Sumner Myers, *Institute of Public Administration*

10:30–11:30 am **General Discussion**

11:30–11:40 am **Summary:** Rapporteur

11:40–11:50 am **Wrap-up:** Presider

3. R&D Budgets and Technological Advance

(Carnegie Institution)

Presiding: John M. Logsdon, *Director, Graduate Program in Science, Technology, and Public Policy, The George Washington University*

Rapporteur: John H. Young, *APS Congressional Science Fellow with the Senate Committee on Commerce*

Significance of advanced technology to the economy—Relative U.S. position, current and trends—Implications for federal funding of R&D—Available alternatives.

9:00– 9:10 am Introductory Remarks by Presider

9:10–10:00 am **Prepared Presentations:**

Lowell W. Steele, *Consultant, Planning for Corporate Research and Development, General Electric Company*

William M. Capron, *Associate Dean, JFK School of Government, Harvard University*

10:00–10:30 am **Invited Comments:**

Klaus P. Heiss, *President, Econ, Incorporated*

Robert L. Stern, *Director, Business/Government Relations Research, The Conference Board*

Michael L. Telson, *Energy Analyst, House Committee on the Budget*

10:30–11:30 am **General Discussion**

11:30–11:40 am **Summary:** Rapporteur

11:40–11:50 am **Wrap-up:** Presider

4. R&D Budgets and Long-Range Planning

(Carnegie Institution)

Presiding: Brewster C. Denny, *Dean, Graduate School of Public Affairs, University of Washington*

Rapporteur: William R. Moomaw, *AAAS Congressional Science Fellow with the Office of Senator Dale Bumpers*

OMB, congressional, and scientific community expectations from long-range planning in R&D—Nature and value of the product—Relation to fiscal constraints and budgets—Role of the scientific community.

9:00– 9:10 am Introductory Remarks by Presider

9:10–10:00 am **Prepared Presentations:**

Hugh F. Loweth, *Deputy Associate Director, Science, Space and Energy Technology Development, Office of Management and Budget*

Ellis Mottur, *Assistant Director, Office of Technology Assessment*

10:00–10:30 am **Invited Comments:**

Russell Drew, *Executive Director of the Presidential Advisory Committees, and Deputy Assistant Director for Special Projects, Directorate for Scientific, Technological and International Affairs, National Science Foundation*

Edward L. Helminski, *Director, Office of Science and Technology, National Conference of State Legislatures*

Glen Schweitzer, *American Society for Public Administration*

Joseph H. Engel, *Head, Department of Systems Engineering, University of Illinois at Chicago Circle*

10:30–11:30 am **General Discussion**

11:30–11:40 am **Summary:** Rapporteur

11:40–11:50 am **Wrap-up:** Presider

12:00 noon Cash Bar for all participants
(Gramercy Inn)

12:45 pm **Luncheon and Final Plenary Session**
(North and South Scott, Gramercy Inn)

Presiding: William D. Carey, *Executive Officer, American Association for the Advancement of Science*

Speaker: Brock Adams, M.C. (D.–Wash.)
Chairman, House Committee on the Budget

Reports from Workshop Sessions:
Workshop Presiders

Reports on discussions—Identification of topics for consideration in future AAAS budget analyses.

General Discussion

Summing up and Conclusion:
William D. Carey

OPENING PLENARY SESSION

William D. McElroy, presiding

Chancellor, UC, San Diego
& President, AAAS

On behalf of the American Association for the Advancement of Science, and particularly its Committee on Science and Public Policy, I want to welcome all of you to this important occasion. As Willis Shapley pointed out in his report, the purpose of that report and, I might add, of this colloquium is to provide, on a timely basis, an informative and critical analysis of the R&D components of the federal budget, with an identification and exposition of the major issues of general interest. We don't expect the meeting to offer a platform for special pleading, although we certainly hope that several critical issues involving policy considerations can be developed and explored from many different viewpoints. I should add that we hope this colloquium can become the core for an annual AAAS science policy meeting here in Washington, D.C.*

Willis H. Shapley

Consultant, AAAS Committee
on Science & Public Policy

Now let me introduce Mr. Willis H. Shapley who will give us a summary of his R&D budget report.

Thank you, Bill McElroy. I appreciate very much the efforts of AAAS and the Committee on Science and Public Policy for making this occasion possible. Or I think I do, because I don't know exactly what's going to happen. In any case, I do appreciate so many of you attending this meeting to help us all learn about the problems of R&D in the federal budget.

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*Willis H. Shapley. Research and Development in the Federal Budget: FY 1977. Washington, D.C.: AAAS, 1976. Hereafter referred to as the Shapley report.

My function has been mainly to put together some raw meat for the discussion, the report which has been made available to you. This is meant to be torn apart; I expect it will be roasted and hope some parts of it may ultimately be digested. My qualifications for doing this report seem to be that I've worked both sides of the problem. I have to confess that in my time I've cut a lot of R&D budgets, but I've also gotten some back. I did a rough calculation the other evening which showed that, while working on "big science" in the Bureau of the Budget, I probably cut more dollars out of the R&D budget than anybody else alive; a dubious distinction, but there it is. I got quite a bit of this back in my ten years helping the National Aeronautics and Space Administration (NASA) fight its battles of the budget, but, by my calculations, I'm still $10 billion short of breaking even. So, Paul O'Neill, the Office of Management and Budget (OMB) still owes the R&D business about $10 billion and I'm sure they will take it in easy payments over the years.

While my report may be regarded as the report of a double agent in from the cold, I should say, more seriously, that in writing it I tried to dust off my slightly rusty license as a political scientist and record some of what I think I've learned in the opportunities and experience I've been privileged to have over the last thirty years in working with federal research and development. At the same time, I was able to indulge two of my main personal interests. One is in the proper and effective operation of the processes of government in the United States, expecially the office of the Presidency, administrative management in the Executive Branch, and the partnership of the Executive Branch and the Congress. The second, of course, is a strong personal interest in, and commitment to, science and technology as such, the excitement of big adventures, both in ideas and in deeds, and the positive uses of science and technology to deal with human problems.

Having said that, let me say a few words about the report. The idea of doing an AAAS budget study has been around for quite some time, especially in the AAAS Committee on Science and Public Policy, notably Ray Bowers, Don Kash, Dixon Long, John Logsdon, and many others. In parallel, Bill Carey and Bill Blanpied at AAAS headquarters, and some members of the AAAS Board had also been thinking along the same lines for some time. Last December a meeting was convened to discuss doing something about it, and it was decided to have a trial run this year. Somehow I agreed to take it on.

One of the problems in preparing the report was that we were trying to move in four directions at once, a little bit like Don Quixote, I suppose. First, we wanted to lay the groundwork for future AAAS budget efforts; second, to give an informative exposition of the budget and the budget process, and R&D issues in it; third, to review the 1977 budget then about to come from the President; and, finally, to consider how we collectively, meaning the AAAS with its affiliated societies, might go about doing this job in future years.

The report, then, reflects lots of limitations. It was a part-time job over three months, and what came out is a very personal statement without benefit of wide consultation or even the normal type of research and discussion that any self-respecting paper writer would normally do. Second, the document is confused in that it's written for several different audiences at once. Also, it is an incomplete document in that it is by no means a definitive statement of issues, or even a thorough analysis of the 1977 budget, because it is based entirely on documents. And, most notably, there are really no answers or conclusions in the report.

This is where you all come in: One of the functions of this conference, I hope, is to advance the discussion, correct some of the ideas that appear in the report, and really lay the groundwork for a better consensus and understanding of the problems of R&D in the budget. To make up for the lack of conclusions in the report, I'll take a couple of minutes now to mention four conclusions that I came to in the course of working on it.

The first relates to the utility of doing this kind of budget analysis -- of R&D budget analysis by an organization outside of the government. My conclusion now, which may be modified after I hear what all of you have to say, is yes, it is worth doing but that we shouldn't expect too much. We have to recognize a whole host of problems, many of which are mentioned in the report. We have to draw a careful line between advocacy of things that we're all for, or that most of us would be for, and objective analysis. We have to guard against oversimplified or implicit value assumptions that some social policies are right just because we happen to agree with them, and others are wrong. We have to be especially careful in the use, and avoid the over-use, of budget statistics. There's such a wonderful plethora of statistics available, it's a big temptation to play numbers games. Believe me, it is not productive. Then, we have to recognize the problem of scale. The budget is really incomprehensively large, like astronomical distances, and the sheer size makes it extremely difficult to deal with it meaningfully, either in total or by selective details. On top of this, there is a real timing problem because the budget keeps getting worked on while it's being looked at. Right now, what I wrote in April has two more months behind it and the situation has already changed. Finally, in my opinion, there's no way that AAAS, or any similar organization, can compete with OMB or the Congress in the degree of depth of attention and skill and professionalism that they bring to the review of the budget. We should not try to.

On the positive side, though, I think there are some real values in an annual budget analysis such as the AAAS might make. The assembly of relevant information of interest on R&D budgets would be useful to many in the AAAS community. I think there is also a real value in independent critical review, provided that it's balanced and not one-sided. I think there is room for and value in focused expositions of selected, significant issues each year. Don't try to address everything, but select some significant issues, whether they're real issues or whether they're issues just because people think they're issues. Then, there is probably also some value in following through on the budget process. Right now there is in the report a slightly obsolete analysis of the fiscal year 1977 budget. It may be worthwhile a year from now, or a year from last April, to check and see how things really did come out on the FY 1977 budget. Finally, out of all of this I would hope would come the basis for some mature and balanced judgments on science policy questions with which all of us are concerned.

The second area of conclusions I think I should address is to answer the obvious question: "After reviewing the FY 1977 budgets for R&D, what do you think?" Well, to try to give a brief answer that recognizes the problems on both sides, I'd have to say that I think the FY 1977 budget was somewhat too tight on R&D. I think the Department of Defense (DOD), the National Science Foundation (NSF), and some parts of the Energy Research and Development Administration (ERDA) got really pretty good R&D budgets out of it but most of the rest of the major agencies, I think, really should have had a little more. To quantify this, let me say that I'm talking about relatively small additional amounts -- with two or three hundred million dollars more (about 1 percent of $24 billion R&D total), spread in the right places, most of the problem areas that I was able to identify would be in quite good shape. And I have to add that I don't think a very good rationale has been advanced to justify keeping the R&D budget quite as tight as it is, considering the total size of the budget, the total size of the deficit, the limits of accuracy in the forecasts of what would happen to the economy, and so on. So, until Paul O'Neill straightens me out, I'd say that it would have been a reasonable outcome for FY 1977 to have had a slightly more forthcoming budget for the R&D areas of the government.

Finally, let me say a couple of words about R&D budget issues. As you recall, much of the report deals with general, continuing, and inherent issues in R&D budgeting. One of my principal conclusions is simply that we all have to recognize that there are real issues, real differences, and real problems; there are no absolute, definitive solutions. If we have a budget, somebody gets money, somebody else doesn't. That is going to happen in any situation; there's just no way of getting around it.

My suggestion would be that we all think about such issues more in terms of improving the process for arriving at good decisions. I don't say "best" decisions because I'm not sure we can ever know in advance whether they are the best decisions. But if the scientific community and the agencies of the government involved can focus on being sure we have a process where responsible people on a thoughtful basis make good decisions, that is probably the best we can do. I should also say that I think it's very important not to place too much reliance on fancy theoretical decision models or systems; R&D budget decisions are inherently too much matters of judgment.

Similarly, my outlook is that we should not place much reliance on overall central planning. As you can infer from some of the remarks in the report, I take a slightly dim view of the more expansive claims or ambitions that are sometimes stated or implied for central planning in R&D. I think decentralized decisions are probably good, some competition is undoubtedly healthy, and even some inconsistencies and a certain amount of confusion may not really be all that wrong or bad in the long run.

I must also confess to doubts on the usefulness of trying to develop general criteria for R&D budget decisions. In my view the question of what criteria should be applied in any given budget decision should be a part of the decision itself. That may seem like an illogical, verbal cop-out, but I don't think it is. I think that the notion that criteria can be set in advance and then applied to the real world as it flows past is incorrect. I think we have to think through each decision just how it should be made and what the considerations on both sides should be.

This leads to the point that it seems to me that the R&D community has a special responsibility to think through for themselves just what criteria they think ought to be applied in various typical situations. Too often in the scientific and technical community the only criteria discussed are how to get more, more, more money, and how to shoot down the reasons against it. I believe the R&D and technical community should take on the responsibility of thinking through from their point of view, keeping in mind their desires and aspirations, just how the total system ought to function. What is really the right decision? Should the R&D budget always increase by 30 percent a year, or 10 percent, or 5 percent, or are there even some conditions under which you should stop doing R&D? These are questions on which I think the OMB and the Congress really deserve responsible help from the R&D community.

In my opinion, it is very important that people who are concerned with the R&D budget problems work to strengthen the basic political and other decision-making processes in government. I don't think that special consideration is really what the R&D community needs. We really need to understand the total situation, work within it, and try to strengthen it. A key point, in my opinion, is to recognize the responsibilities and strengthen the leadership and management of the line agencies which have the responsibility for getting the government's R&D jobs done. Central staff agencies like OMB, and the Office of Science and Technology Policy (OSTP) when it comes back into existence, have important functions and their problems and point of view have to be recognized and understood by all concerned. But I believe their role should primarily be to support and strengthen the decision-making process of the line management agencies, except in special cases such as if an agency completely defaults on its responsibilities, or help is needed in establishing a new organization or program, or something like that. In all this, I think the scientific community and all concerned should recognize Congress as a full partner in the process and not regard them, as sometimes seems to be the case, as somehow the enemy or the bad guys or the people to be convinced or manipulated in the process. We should recognize and help strengthen them in the discharge of their functions, but at the same time watch closely the proper dividing line between legislative and executive functions. If that is done, I think the whole system will be stronger for it.

Now the last question I want to discuss is what should be the role of the outside community, the science and technology community, as embodied by the AAAS plus its affiliated societies. I think we have to work here towards understanding and behaving in a role which I would call responsible advocacy. In our society generally these days, I think and hope that more and more of the special interest groups are coming to recognize that just a sheer advocacy of their short-term, immediate and traditional interests is not the responsible way to go. It is not really the productive way to go, either. I see, or at least I hope I see, for example, in the recent behavior of some public service unions and other organizations who are in positions of great power and influence, positions to do both good and harm, some beginnings of a recognition that they have to exercise the powers of their positions in a responsible way for the good of the entire society. I would hope as the AAAS continues, if it does, with its study of R&D in the federal budget, that the AAAS and the scientific community could evolve into a role of responsible advocacy, which takes account of both the interests of the science and technical community as they see them and, at the same time, the broader interests that are epitomized by the President and his Administration, the OMB and the Congress.

Thank you very much.

Thank you very much, Willis. I did note a little bit of idealism slipping in near the end of your presentation when you suggested equal responsibility between the executive and the legislative bodies. I agree with that in principle though, as we have seen in certain agencies, such a division of responsibility does not always work too well. I hope some of our long-range R&D planning groups will turn their attention to the problem of making that division more meaningful.

H. Guyford Stever
Director, NSF

Our next speaker really needs no introduction. In the latest Chemical & Engineering News he is quoted as saying that it has been a tough four years. I think I can agree with him. At least, having observed the Washington science policy scene from a great distance, I can appreciate what Guy Stever has endured these last four years. As you know, he became Director of the National Science Foundation on February 1, 1972, and soon thereafter he also became the science advisor to the President. Those two capacities are full-time jobs for at least two-and-a-half people. Everyone agrees that he has done well in both of them. We are very pleased and honored to have with us today Dr. Guyford Stever, Director of the National Science Foundation.*

It is always a great honor to address an audience all of whom know more about the subject than I do. The wisdom Willis Shapley displayed in his review of the R&D components of the federal budget is so great that I think the best thing that could happen for the rest of the afternoon would be that we sit quietly and ponder what he has said rather than discuss it.

Willis' opening remarks contained a statement that I couldn't understand at all, frankly. He said that when one goes through a budget-making process with all the hard decisions that have to be made, inevitably some people are satisfied with what they get and some are not. Well, over the last three years I've supervised the disbursement of about $3 billion of federal funds, and I have yet to hear from anyone who has gotten anything!

As you know, I have tended toward the R side of the budgeting process and what I say may reflect that bias. Along these lines I would like to discuss a number of issues, as I see them, involved in considering the making of a research budget.

---

*Zerkel, Fred H. "Stever Reflects on Four Busy Years," Chemical & Engineering News, 54, No. 23 (May 31, 1976), 12-13.

The first of these issues has to do with maintaining continuity and strengthening research. Most people feel basic research is vital and that it is important to continue its advancement. A certain level of support in all fields of scientific research seems to be necessary. That support can't be turned on and off on demand without jeopardizing several things: first, the information flow within the field itself; second, the life of the important institutions which husband that research; and third, the flow of good talent into the field. However, it is often difficult to convince outsiders that all disciplines are making important contributions when one field receives more public attention than others.

The increasing view in the Office of Management and Budget (OMB) that the National Science Foundation (NSF) should act as a balance wheel among the various fields of basic research has come to the fore this year with the President's FY 1977 budget decision to strengthen NSF. As I have already noted, we must keep the institutions that husband basic research strong. But there is competition among the academic institutions in this country. The real question is which ones are to be kept strong? Or are we to let some go up and some go down? Some countries have made very clear decisions about the number of institutions and the number of people that they will support. I have often mentioned that at NSF we have very clear records on the batting averages of those institutions that the science community itself has judged as being the top ones. But no institution at the top is guaranteed to remain there.

The problem of maintaining continuity and strengthening research is a complex one. It is not just a matter of deciding that a certain research center is a good one and to keep it that way, because that very thought may start its decline. On the other hand, it is important to have a mechanism in government to take up slack when an agency decides that it can no longer support an important type of research or an important laboratory. NSF has tried to do that in a number of cases.

The second issue I want to discuss has to do with anticipating and supporting ripeness in research. The very fact that the first issue has to do with continuity may turn us away from this business of the ripeness of research. If too much emphasis is placed on the first issue, it may not be easy to recognize when a field has arrived and really needs special help. That judgment, the selection of the fields that are ripe and ready to burst forward is a very important one. This country hasn't done too badly in putting together budgets to encourage such fields. I think of the build-up of molecular biology in the early 1960's, for example. Again, beginning in the mid-1960's there was a feeling that many of our research institutions weren't connected well to our society, and that interdisciplinary research needed strengthening, so we moved in that direction. Of course, there are many other public concerns toward which we have moved; pollution control, energy, and so on. Right now, for example, there are a couple of pressure points. One pressure point concerns earthquake prediction. Scientists in that field seem to have brought it to a point where it must be looked at very carefully. Also, the potential for significant neuropsychological research in the late 1970's and the early 1980's is considerable, and we're beginning to provide special support to that field.

At this point it might be useful to note that the development of two major fields of science during the past fifteen years have given us so many opportunities and so many new thoughts that they have changed our approach quite a bit and have strained the budget. The whole biological science field has grown rapidly in many complex ways, and the space field has given us a different outlook on what we are and how we live on spaceship Earth. Developments in both fields have forced us to look at a whole new range of approaches to the funding of basic research.

Let me turn to the third issue: that of relevance and the need to fill in knowledge gaps revealed by applied research and development programs. This issue is only peripherally related to the development of material things for our society. That part of our society which actually produces those things works pretty well. The profit motive is a good one, and the market system a pretty good pressure system to force our applied research organizations to work in areas where knowledge gaps need to be filled in order to produce a product.

But, on the other hand, there are some nonproduct related areas where we need to fill in the missing elements.

Consider, for example, environmental problems involving changes or nonchanges in the ozone layer as a result of man-made products. Where are the pressures on the private sector that would induce it to conduct research to obtain practical data so society can make important decisions regarding this problem?

As a second example, in what conceivable ways could the profit motive operate to induce the private sector to perform much needed research in the earthquake prediction field?

The energy technology business is an example of a field in which the profit motive operates to induce private industry to undertake a portion of the needed research, but by no means all of it. Some parts of energy technology are so intimately involved with the private sector that the requisite applied knowledge is filled in very quickly. Yet there are other parts in which this isn't true at all because the return on investment is so distant private industry is reluctant to work on them.

The fourth issue I want to lay before you relates to the problem of inflation and the rising cost of scientific equipment, particularly in big science such as astronomy, high energy physics, oceanographic and atmospheric research. Of course, inflation escalates the cost of R&D, but it is hard to estimate the cost over the long range, even though budgeteers like to try. The problem with making long-range estimates for basic research is that there is no well defined output product. In other words, society is not telling us that we need so much basic research, no more, no less. We simply do not know how to quantify those needs. The fact is the liveliness of basic research will place us in the unpleasant position of trying to determine how much we should be doing at one time in a given field because we can always do more in many different fields. Of course, we are still a very powerful nation in research and yet we know there has been some decline in the constant dollar input into basic research. We hope to relieve the pressures with the addition of $50 million in the President's budget for FY 1977. But there are many things we can't predict. The impact of rising electricity costs on accelerator operation and rising oil costs in oceanographic research can change the level of our research efforts quite a bit.

The fifth issue I want to consider concerns political pressures, pressures based on special interests and on special geographic considerations. Both the public and the private sectors are putting pressure on NSF and other federal agencies to support certain lines of research: for example, coal research and environmental research. As you know, there is intense competition among many regions of the country for the proposed Solar Energy Research Institute. And there are pressures to support educational research institutions in certain regions.

Internationally, we are in a curious position with respect to politics and science. It used to be that science and technology produced products that could be sold overseas. Now the very knowledge in those fields is a product that is sold overseas or is used as an important part of international political dealing. This country has made a number of bilateral agreements during the last few years to handle what are essentially political problems. The maze of these bilateral science and technology agreements is itself amazing, but I won't pursue that point!

The sixth issue concerns manpower and the generation gap. In my opinion, this issue is one of the most serious ones facing our society. While we were in a period of constant growth in science and technology, the manpower problem was nonexistent because every scientist and engineer who graduated got many, many offers. But we know that today the situation is unstable. During the last half decade we have experienced a difficult period and have scars to show for it. Yet the time scale in the education of young people is very long and it is hard to know whether scientists, engineers, and technologists should be given additional advantages in the free market situation that exists in this country with respect to their choice of field. The other day I saw an article about a problem in France where the same thing happened to a fine group of young French men and women who had been selected to be airline pilots. Each had about $120 thousand worth of training but when that training was completed, it turned out that only half of them could be employed. We are trying hard to achieve a better balance. But I think society has done a poor job of mastering this problem.

The only thing harder to predict than the supply of students at a given time is what the long-term manpower needs will be. Of course, NSF and other agencies have graduate student support programs. But, at least at NSF, these are primarily aimed at the best students and do not address numbers problems at all. We are trying to reshape some of our programs so that students get an early introduction to research and to the real doing of science. We think this might start the self-selection process a little earlier, by letting the students themselves decide if they really want to continue in research courses.

When a special national need is discerned, we do shelter some special education programs. With the current energy problems facing this country, we have introduced programs to train young engineers in energy research.

The seventh issue concerns science and participatory democracy. One of the primary reasons I am happy to be here today is that this meeting provides one more indication that scientific societies take this issue seriously. The growing pressures for openness, accountability, risk reduction, immediate payoffs, and attention to social values have changed the conduct of the scientific research enterprise considerably. Frankly, if we were to depend upon public servants to handle these new movements in today's society, we would be inundated. Unless the scientific and technological communities become involved we haven't a chance of matching the interests of science and technology with the interests of the public. At NSF we will be working a long, long time adjusting to the new laws and regulations in this area. We can't possibly do all of the thinking that is needed by the scientific community.

Of course, we have our public understanding of science program, a new science for the citizens program, and various course content improvement programs. But we cannot satisfy societal pressures in these areas through these programs alone. The entire body of science and technology must become involved.

The eighth issue concerns the coordination of federal R&D programs. I believe we can do a reasonable job here. The OMB has made a strong start and will be assisted by the reestablished White House science advisory mechanism. On the other hand, I still believe in the pluralistic approach to scientific research and development; having different kinds of governmental institutions support research in the private sector, and supporting some research within the government itself. I don't believe in coordination before the fact. The coordinators have to face problems that are generated by different kinds of agencies first. That makes their job very tough. But Congress has passed good legislation recently and presumably we will be able to do a better job than we have in the past.

Issue nine relates to the equal opportunity aspect of R&D. Each year NSF and every other science agency tries to do more in this area. But we have a long way to go to provide improved opportunities for minorities and women in science, and to make better use of talents of the handicapped.

The tenth and final issue I want to lay before you concerns the problem of balancing near-term and long-term goals. Our society is set up with almost unbelievable pressures on the near-term side. The success of corporation presidents and political leaders is always judged on a short-term basis. Yet it is important to give the short-term decision-makers of twenty, thirty, or forty years from now the new information that they will use in their own short terms. That is our greatest problem. It is a tough fight to keep up the pressure for balance, because there are many pressures forcing us the other way. I would say, in my time in government and in my time in science, this country has grown more aware of the importance of the long-term part of the job. Not all decision-makers have gotten completely up to speed, however. I remember testifying to a congressional committee in which one member didn't seem interested in what I had to say. I was describing how, when the new thousand-foot antenna at Arecibo was upgraded for shorter wave lengths, there was a joint dedication between the National Aeronautics and Space Administration (NASA) and NSF. I related to the committee the research program of sending out messages to the nearby star clusters and looking for return signals. This particular congressman got very excited when he realized we would be sending out intelligible messages in an effort to communicate. However, when he learned that time of transit out was 24,000 years, and that an equal return time would be required, he lapsed back into disinterest.

In conclusion, I think we have made considerable progress in coming to terms with all ten of these issues during my time in science. But we have a long way to go. As I have already noted, I think the biggest difficulty is to establish better relationships between the body of science and technology and society as a whole. I think this will not be accomplished by the efforts of a few people who testify once a year on the subject. I think it is the duty of the entire science community. I congratulate you on the start you have made by organizing this colloquium.

Thank you.

Thank you, Guy.

Paul O'Neill

Deputy Director, OMB

I think most of you will recognize that out in the field we regard the Office of Management and Budget as the tough guys. They are the ones who have to make the really hard decisions because I can assure you every agency head will come in with requests that probably exceed the federal budget by at least a factor of two. So OMB has the thankless job of having to judge what the first priorities for funding are. In other words, they have to deal with the perennial problem of overseeing and allocating limited resources.

Our next speaker, who apparently thrives on a steady diet of these problems, probably knows more about the federal budget than any other person in Washington. We are pleased that he has been willing to come here today and talk about his problems. The Honorable Paul O'Neill.

I'm delighted to be here. It has been my impression over the years that there is still a great deal of mystery about how Presidents shape their programs and what considerations they take into account in doing so. Willis Shapley's fine, analytical report performs a great service by laying out the steps in the budget formulation process so that everyone can gain some insight into them although, as he has indicated, it is very difficult to keep up with the changing process. With the advent of the new congressional budget process, there are signs that there may be further changes in the way our budget and resource allocation process work. But Willis' document should provide a basis for understanding those changes.

In commissioning the Shapley report and arranging this colloquium, the AAAS is doing something that has great potential. I understand there are a number of people here from Congress and the Executive Branch, as well as the R&D community, and that is very good. Quite frankly, I feel that we talk together too infrequently about what we are trying to accomplish and about our individual gripes. From time to time I talk to groups throughout the country and after I have spoken people say to me, with funny looks on their faces, "You know, that makes sense. You're not such a bad guy after all." That is nice, but suggests that we don't do enough reasoning together. Therefore I applaud this timely opportunity for reasoning and talking together, and for working to understand better the individual perspectives that lead to the conclusions we draw.

The Office of Management and Budget (OMB) is once again in the process of budget formulation -- this time for fiscal year 1978. We also have the new Office of Science and Technology Policy and a new two-year study committee coming into being. I hope these important developments can be influenced by the discussions that take place at this colloquium. One regret I have from watching the federal process over the last ten or fifteen years is that we do not seem to have found a way to learn very well. We begin the process, but we don't take the next step forward, based on what we've learned earlier. I am optimistic that our discussions here can be a learning experience.

Let me comment on something Guy Stever said concerning the subject of budgets and budgeting. He noted that he has participated in the distribution of some $3 billion since he has been in Washington -- but hasn't found anyone who has gotten any of that money. That remark led me to reflect on my own years in the Bureau of the Budget and OMB. I was horrified to realize that I had participated in budget formulation for an amount approaching $2-1/2 trillion, and I too have never met a single, satisfied client. I'm sure there are some, but I haven't met them.

Before going directly to the subject of R&D budgeting, it's useful to have a context, which Willis' report gives us. R&D budgets are not made in a vacuum. In my experience, the preparation of the federal budget this year showed the most detailed and particularistic involvement of a President that I know of. There may have been other Presidents equally involved in their budgets and perhaps Bill Carey participated with other Presidents the way we did this year. At the outset of the budget process early last spring, one dimension that was very important to the President, as it had been to previous Presidents, was to ascertain where our fiscal policy, our total federal spending, was headed in the context of the overall economy, taking into consideration the trends of unemployment and inflation.

I think that context is important to keep in mind if the input of the scientific community into the preparation of the R&D components of the budget is to be useful and meaningful. This year, as I'm sure you know, the context was a very difficult, disciplined approach in an attempt by the President to put our national economy back on an even keel, back to what he felt was a stable growth path that would return our society to a position where it would produce at or near its maximum potential without going through the roof because of overstimulative fiscal policies.

Beginning as early as last April and May, there was a very tight constraint in the way the President approached this budget, and that necessarily had an implication not only for science, research, and demonstration activities, but for every other area as well because, in that broadest of federal budgeting contexts, every dollar spent for a particular activity is a dollar not spent elsewhere. The competition in the Oval Office for federal dollars, no matter how many there seem to be, is fierce and the claimants legion. Even in a budget of $395 billion, there seems to be no end to claimants who feel, as Willis indicated he did, that some additional money would be very desirable. As a matter of fact, when Willis said he thought there ought to have been another $200-300 million in the R&D budget, I thought to myself, if that budget had been $200-300 million higher he still would have suggested the same thing.

Within R&D itself it is important to recognize that there is no self-contained R&D budget in a classic sense: That is, we do not propose $24 billion for R&D and general science community activities, and then decide how to distribute it among the claimants. This does not mean that the President, OMB, and the National Science Foundation (NSF) are not concerned about the totality of federal funding for R&D and science, but that we don't start out with a magic number for R&D.

As a matter of fact, we don't start out with a magic number for any particular area. Rather, the President gives us an indication of how much federal spending he thinks would be desirable in the broader context of fiscal policy, and he then makes some partition decisions, department by department and agency by agency, on the basis of available information which indicates what areas need automatic funding and what some of the marginal implications are -- a plus or minus in any particular area or activity. But, at the time budget formulation is started, there are more loose targets than there are firm goals so, even if one were to go through and take the targets that are given to the departments and agencies, one would just have a general sense of direction at the early stage of budget development. If one were looking at R&D, it would not be possible to get a clear impression where the planning was headed and where the numbers were likely to come out.

I think OMB sometimes confuses people by issuing a special analysis on research and demonstration activities in the federal government. It's important for you to know that that analysis is done after the decisions have been made. In other words, it is a report on what the budget contains, not on the context in which decisions were made. That's an important distinction to keep in mind, but it does not mean we are not concerned about the totality of R&D. Over the last ten or fifteen years OMB has, of necessity, become more concerned about what's happening in federal R&D because, as a result of the profusion of federal R&D being done, there have been some monumental secondary and tertiary effects not directly related to the original objectives of individual R&D projects. By that I mean the federal government has become more important in terms of institutional support of R&D. As the federal government's investment in certain areas has increased, many institutions have, in effect, substituted federal funds or federal relationships for those that existed before. There doesn't seem to be any turning back once a particular institution or kind of activity has been weaned into a federal grant situation. My own observation is that it's very difficult for people to return to their institutions for support when federal support ends because those institutions will have other pressures and plans that do not include them. Then it's back to the federal government.

One of the consequences of those secondary effects is that the federal government has become more concerned about R&D because it has heard from the scientific community how important it is for there to be continuity and stability in federal funding. And another is that the scientific community has become dependent on the federal government. I think that's a very important effect. I don't think that when this trend started many people really understood its institutional implications. It is important that we do try to anticipate institutional consequences. My own observation is that it will be very difficult to undo some of those secondary effects even if we want to, and I think that as scientists, legislators, and those in the Executive Branch go forward together we should try very hard to anticipate where we are going and what those effects are.

Let me now turn to some specific problems I see in dealing with R&D in the federal budget. First, there are great uncertainties about the statistical, budgetary information that we have to deal with and I'm sure many of you are familiar with this problem. In looking at almost any department's or agency's decisions or recommendations in the budget process and the definitions used to convey what they are doing, one will find that there are great differences between departments and agencies. In other words, what some people call basic research, others call applied research, and others call development. In part, it's a game: it depends on what's selling that particular year. But this creates uncertainty and concern for those who try to shape decisions and help the President shape decisions about R&D.

That great uncertainties exist is due, in part, to the status of our R&D community. That is, we do not know very well how to measure where we are and where we would be if we had one level of funding or another. In all these things, we don't understand very well where the margin is and what the implications are of changes in the margin.

In the same vein, there are great uncertainties about balance. I don't think any of us believes we have a good calculus that tells us how federal funds should be distributed between fields of science. We don't know. It's very difficult to find a calculus that's satisfying and gives us some assurance that we're doing the right thing.

I don't think any of us has a very good sense of what the correct rate of growth for R&D is, if any. That's another big uncertainty we must deal with. I don't think any of us has a firm sense about the appropriate distribution of funds between, say, NSF and the mission-oriented agencies. And so we are flying by the seat of our pants in that area. In the medical and health research areas, I don't think any of us has a sense of the right allocation of resources between categories of disease if, in fact, that's even the proper way to look at the problem. I don't think any of us has any great certainty about how to measure the payoff, or potential payoff, of federal investments in these areas, or if they should be measured.

Then there's another kind of uncertainty -- that of ultimate social utility, a different concept than payoff. By payoff I mean profit, and by utility I mean a usefulness to society that can't be measured in dollars.

We also have a great uncertainty about where the margin in investments is between the federal government and the private sector. We're concerned that those things with potential value for society be done, but we don't want the federal government to take on those activities that would be done in any event. And we don't know where that margin is. We do not even know how to think about that margin, but we are concerned about it. Then there's an uncertainty of what is meant by a proliferation of programs. We don't know how many different programs there ought to be, or how many different agencies or departments are the right number. It has been suggested that there is some value in conscious duplication, but again we don't know that.

Those are the kinds of uncertainties and imponderables we find ourselves faced with. They are not just OMB problems. Ultimately these are society's problems, even if OMB is the scapegoat. They are difficult intellectual problems that need to be faced and that's why I'm delighted all of you are here and thinking about them.

I am convinced that there are research and development and science areas that must be supported by the federal government because I don't think they will be supported elsewhere, either because of their market characteristics or for other reasons. I also believe the scientific community and the government community must find a better way to communicate with each other about the uncertainties I've described. In the time I've been in Washington, I've seen the competition for public funds become increasingly fierce despite the fact that the gross national product has grown by leaps and bounds. And, although we're still a long way from perfection in other areas of federal budgeting, we are making some progress in dealing with these uncertainties.

In some areas that's not too hard to do. But it is an uphill battle to take science and R&D recommendations to the President without a fairly good intellectual construct. I for one don't think that the science community should be willing to let the future of science research and development in this country depend completely, or even largely, on emotional appeals for continuing or increasing levels of assistance. I have no magic answer for you. I wish I did. But we all must struggle with how we can push back these uncertainties so the competition will not get out of kilter as various interests compete for available public funds.

Let me repeat that I am very happy to be here. I am happy all of you are here. And I am hopeful that you can help us in our jobs so everyone will say that OMB did it just right this year. Thank you.

Thank you, Paul, for a very thoughtful statement. I'm sure that many of the top hundred or so research universities that turn out most of this country's Ph.D.'s would agree that we may already be too far down the road in our dependency on federal money. But I for one don't see any turning back. No doubt we could become more sophisticated about how we transmit our ideas to OMB and to the lead agencies on what we think needs to be done.

Paul Doty

Director, Program for Science &
International Affairs, Harvard

Our next speaker, Paul Doty, has agreed to make a few remarks about budgeting from the university standpoint. Paul is an outstanding biochemist who is also very much interested in the broader problems of the investment in R&D in this country, particularly from the university standpoint. It's a pleasure to have him here with us today.

I would like to add my compliments to those already given Willis Shapley. We certainly owe him a debt of gratitude for the fine report he has written.

I will discuss my view of the "elephant" we are all describing, with emphasis on basic research. As you know, basic research support is a relatively small part of the federal budget. Nearly everyone agrees that about $100 billion of the federal budget is adjustable from year to year. Since the R&D budget is about $25 billion of that category, it appears that R&D is an enormous part of the adjustable budget and, of course, it is. But I will talk about the part that is only 10 percent of that $25 billion, of $2½ billion. That amount is not small, I admit, but neither is it catastrophic in terms of small adjustments that might have a large payoff.

First I want to address some larger views on the subject, and then some smaller, more specific ones. I will begin with a few remarks on the budget as a function of time, the situation scientists find themselves in as a consequence of the changing profile of the science budget, and the consequences to science itself.

It is useful to keep in mind our four decades of experience in federal budgeting for science and technology. The first of these decades was during World War II, when the use of science for a national purpose got its first real demonstration. The decade after the war was one in which there were great expectations that strengthening our science and technology capability would allow us to solve society's problems with the same success we had during the war. In the third decade, the first decade of Sputnik, we experienced a period of heavy growth. During the fourth decade, the one just ending, we have had a mild budget recession. More specifically, in the first Sputnik decade we doubled constant dollars for federal R&D. The early sixties were an exciting time and it seemed safe to assume that a 15 percent annual growth of R&D was a God-given right. But that situation changed quickly and in the last decade, as Shapley's report stresses, we have seen a decrease of 20 percent in federal R&D.

Basic research support fluctuated even more. During the first Sputnik decade, 1957-66, federal support for basic research increased about fourfold. In the last decade, it has decreased about 20 percent. It is interesting to note that industrial R&D has not followed this same pattern, but has increased, albeit more slowly, in this period. During the last decade it increased about 25 percent. Industry's commitment to basic research has remained constant and its funding of R&D has greater stability than federal funding. And if we look abroad we see that most advanced industrial countries in the world recouped in the decade of Sputnik, but in the last ten years they have made enormous investments. In West Germany, for example, both federal and industrial R&D expenditures increased fourfold between 1963-72. Each country marches to its own drummer of course, but there is little that is more different than the relative spending on R&D in the past decade between West Germany and France, for example, and the United States.

What about the scientists? I will speak only of Ph.D.'s, not because they are special, but because we have better statistics on them and because what happens to Ph.D. scientists is indicative of what happens to non-Ph.D. scientists.

In 1974, 45 percent of the Ph.D.'s in science and engineering worked in R&D. They numbered about 100,000 and half were located in colleges and universities.

The production of Ph.D.'s peaked in 1971, but they are still being produced at a very high rate, about 13,000 per year in science and engineering. Ordinarily about 6,000 of these would enter R&D. Assuming that about 2,000 Ph.D.'s leave R&D each year, the entrance of 6,000 Ph.D.'s would mean a net growth of 4 percent in R&D manpower at the

Ph.D. level. This seems to be a normal growth situation, but it has occurred in a decade during which budgets have decreased at about 2 percent per year. This is the simplest way to characterize the basic dilemma and mismanagement of the past decade: a prolonged, mild contraction of the federal R&D system, and a continued production of people most eminently fitted to enter it. A difference of about 4,000 Ph.D.'s a year accumulates who cannot be accommodated in the national R&D effort. So the environment in which science is performed is increasingly affected by an imbalance that is unrelieved, except for improvements in this year's budget and economy.

The most subjective question is how science itself is faring. We don't have statistics like those for Ph.D.'s. My impression is that R&D performance in industry is the most difficult to evaluate, but there are some indices which show it declining. Cost overruns are increasing and there is a slowing of identifiable innovation that characterizes an aggressive and growing technological society. Whether this is merely a consequence of economic fluctuations and will disappear with an improved economy we do not know. But the output from U.S. laboratories is still extremely good, particularly at the growing edge of most sciences. On the other hand, this must be contrasted with a very substantial resurgence of European science and, to some extent, Soviet science. The gap is narrowing.

We have no clear view of the dynamics that apply to the generation of scientific knowledge. I suggest, however, that we are seeing a sequential process in which funding of basic science reached its peak in 1971. Critical discoveries made in U.S. laboratories may be reaching their peak now. U.S. contributions to forward motion on many scientific fronts will go on, but ultimately they will almost certainly decline if the present rate of contraction of R&D continues.

Looking at the smaller, more discrete aspects of the situation, I would guess that if one asks a young scientist who has not read Willis' report, but has absorbed a certain amount of myth and has a certain amount of his own clear, but naive analysis, for his views on the budget scene, he would probably agree when we say that there was a period of too fast growth in 1958-67. But, if he's knowledgeable, he knows that was primarily a response to the Cold War and, in any event, he was too young to be responsible for decisions made then. I think he would wonder why, if this were an aberration to be corrected, the correction has taken so long. He would wonder why almost every item in the federal budget has grown over the last ten years except real support of R&D. He would ask how long this contraction should continue. And this would bring him to the heart of science policy and budget planning.

Basic research is the seedbed from which engineering and development proceed and it requires special husbandry. How fragile is this seedbed? Can it be readily rejuvenated after ten years of negative growth? What is the timing? And, ultimately, how does one find the proper balance for society's investment in R&D? Of course, the answers to these questions depend on the goals society sets for science and it requires a great deal of dialogue. But surely the situation can be improved. Is it not possible to put emphasis on assessing budgets after five years, to look at their results with as much care as we devote to five-year projections? Shouldn't those who petition for a piece of the science budget be held more visibly accountable? These are attitudes toward the budget that ought to be considered.

And what of the situation of scientists? One cannot have lived through this last decade without seeing younger scientists become involved in a conflicting situation in which, while the growth and excitement of science show no decline, the conditions required to do science are contracting at a rate of more than 2 percent per year. Salaries for Ph.D. scientists have decreased relative to salaries for those who hold other advanced degrees such as medicine and law. The rate of advancement scientists can expect in universities has greatly diminished. For example, a survey of six top university physics departments in the U.S. shows only one tenure opening in all six departments in the next three years. This is one aspect of the serious university problem of aging department faculties caused by the decline in the hiring and promotion of younger people. No one knows what a physics department will be like when all its faculty are between the ages of 50 and 65. But that is the destiny of many departments if the present level of support continues for another fifteen years. The physical environment is changing too: Buildings are becoming obsolescent and private and federal funds for renovations are nonexistent. The rate of new building is greatly diminished, perhaps a tenth of what it was in the previous decade. And universities receive only relatively modest overheads on federal grants that do not allow for correcting such deficiencies as do the much higher industrial overhead arrangements on government contracts.

Finally, the university scientists' instruments are no longer modern. Over the last ten years, National Science Foundation (NSF) appropriations for permanent instrumentation has diminished by 50 percent in real dollars. Project support levels are about half of what they were ten years ago. There is an increasing contraction of the range in which agencies will support research. There is a growing concern that ideas that do not have obvious relations to federal programs do not get funded. And there is the difficult and somewhat embarrassing problem of research funds being diverted to the support of education itself or, in medical areas, to patient care.

These are just a few of the problems faced by the basic research community. Furthermore, the problem of fluctuation in the level of funding persists despite efforts to solve it. There are cases where extremely small incremental costs would double or triple the research capacity of systems already built. For example, the SLAC linear accelerator is operating at only 31 percent of its potential. A modest increase in funding would allow it to operate at 80 percent, and that increase probably could have been provided from savings in its budget, if it had been given a steady budget during the last few years.

The problem of vacillating support is even more serious at universities because it is there that students decide what to study and where they think science is headed. Since universities must cope with the problem of finding support for a rapidly maneuvering body of students, they are more vulnerable to the problems of decreasing budgets. Decreasing budgets have turned many promising students away from science. The situation certainly does not deter the most determined and the brightest, but the troops on which steady scientific advance depends are decreasing in number, and probably in quality.

Despite our uncertainties about how our system of science and technology works, we do know enough about the ecology of promoting human activities to be able to program the desired level, provided proper support is forthcoming. The question, then, is what does our country want in the long term? The answer depends on the purposes that society wishes served. Are we to remain the leading technological country on this planet? Does this require a growing commitment to basic research or, conversely, can our economy and our spirit survive a continuing decline in science and technology? These questions go back to the basic imbalance mentioned earlier: the 4 percent net growth in new Ph.D.'s trained for R&D confronting a system that has endured a 2 percent annual contraction during the last ten years. I think it would be interesting and useful to have studies that attempt to foresee the consequences of several alternatives for the future.

For example, if we had known ten years ago of the coming decade of mild contraction in R&D, how would we have handled it? Would it have been better gradually to close half our graduate schools so the remaining half could continue to enjoy a relative affluence? I think the net product in terms of science would have been better, but it is another matter whether society would have been better served. If we face another decade with the possibility of no growth or mild recession, how would that best be handled? Can we find an adequate and acceptable way to decrease the rate of doctorate production when our system, unlike that of almost every other industrialized nation, rewards individual incentive? And, conversely, can we not reflect on what might have occurred if the contraction had not taken place, but rather a stable 5 percent growth had begun in 1967? If that had happened, we would have twice the R&D commitment we do today. If we propose a growth rate of 5 percent per year for the next decade, are we prepared to cope with its consequences? And, unless we go that route, can we justify the continued production of Ph.D.'s at current levels?

I will close by referring briefly to a different set of problems that deal more directly with the way government handles science and the budget process. I've already alluded to one: a reexamination of agency responsibility for support of research in a broad sense. Are agencies operating at the proper level and are they making their support available to potential clients?

International science presents a different set of problems. Less planning has gone into this area than into any other part of the science support system. There is a hierarchy of international involvements ranging from the classical support of individual exchanges, through study commissions and joint ventures, to the theoretical possibility of resource sharing and substantial joint developments. But have we really considered sufficiently the possibilities of resource sharing to join in international adventures?

International exchange has been going on for fifteen years or so, and has its own rhythm. But it's being questioned now. We don't know if there should be more or less exchange. Joint commissions and joint ventures that result from agreements by Secretaries of State have increased until now they number forty and most of these have been agreed to under our present Secretary of State. But they do not have proper budgets. Indeed, it is difficult to find just where the money to support them appears in the federal budget.

Wouldn't it be proper for the State Department to review its foreign policy vis-a-vis these kinds of relationships? But how can that be done unless the State Department itself has a science budget sizeable enough to allow it to make a contribution and, therefore, share in real projects? With the State Department budget now at a billion dollars, isn't it possible to reallocate a few million for that purpose? It seems to me there is a disjunction here between policy and budgetary support that is as discontinuous as any in the government. And, as science and technology interactions with foreign countries become more decisive and grow larger, it is only by strengthening this process that we will compensate for our lack of central direction that deprives us of many possibilities.

The fundamental question, however, is whether or not we have an oversupply of scientists. Will scientists remain in a poor position as clients to the federal government because the level of R&D activities is such that they are on the verge of unemployment? Or will we find a way to a consensus between the level of R&D and the rate of production of Ph.D.'s, even with a ten-year lag in the process? This forces the alternate question of how to find the proper level of R&D spending in a complex society. As matters now stand, that is left to scientists and engineers who are in a losing position on the economic scale:

a very poor posture from which to be an advocate. But who, then, is to speak for the other side? Who is to argue that the U.S. must continue to play a leading role in science and technology, and see that it is wisely used both at home and abroad for short-term problems as well as the propogation of what we continue to think of as one of the mainstreams of western culture? Clearly, a great deal remains to be done.

Thank you.

Thank you very much, Paul, for providing us with a very different viewpoint from the other speakers. A number of administrators in my position are becoming more concerned about the health of the institutions that turn out our top research people. We'll have opportunities to explore some of the points you raised during the remainder of this colloquium.

James W. Symington

Chairman, Subcommittee on Science,
Research & Technology, House
Committee on Science & Technology

I am sure the next speaker needs no introduction to this audience. As you know, he is one of the primary supporters of science and technology in Congress. When I was Director of the National Science Foundation, I always knew I'd better be carefully prepared when I appeared before his committee. He did his homework very well indeed and could ask very critical questions. It was always a pleasure to work in a cooperative way with Representative James Symington, and we are very happy to have him here today

I would like to congratulate the American Association for the Advancement of Science for calling this meeting on "Research and Development in the Federal Budget." It is timely, and it offers the promise of major contributions to all who are involved with research and development in the United States today.

However, after reading Willis Shapley's report, which was prepared as a basis for this colloquium, I am not at all sure there is much that I can add. Willis is an old friend, dating from his time with the National Aeronautics and Space Administration, and I am glad that his retirement was not in the literal sense.

Relationships of Science and Government

Over the years the AAAS has played an important part in developing closer ties and better understanding between science and government, but from time to time there have been major turning points which deserve special attention. I am particularly pleased to participate in this meeting because I think it marks one such turning point. By this I mean that your Association appears to be moving toward a more activist approach to analyzing and influencing national science policies and the research and development budgets of the federal government -- and of the nation as a whole.

It may well be that this meeting will rank in importance with your December 1934 Pittsburgh meeting which marked a significant change in the way scientists looked at working with government. When one remembers that the prevalent attitudes about science and government among scientists of that day were fear of being contaminated by becoming involved with politicians, and fear of political control of science if government provided funding support, it is remarkable that the AAAS got behind its new president, Karl Compton, and the "Program for Putting Science to Work" that he had proposed to President Roosevelt.

Although the specific proposal was not approved by the President, federal science budgets were increased and there emerged a growing consensus in the 1930's among some scientists and politicians that they should work more closely together. This latter development was no small achievement -- and I need not recount how vital to our national security such relationships became as the storm clouds of World War II broke upon the world. President Roosevelt had developed high respect for scientists and he entrusted great responsibilities to them in the war effort.

I think it is significant that the nature of the science and public policy questions that began to emerge in that period have a very modern appearance. Several of these policy questions involved: 1) suggestions that the United States participate more fully in the international scientific community and that it draw to a greater extent on the world reservoir of knowledge; 2) attempts to counteract the frugality of Congress with respect to science; 3) advice designed to eliminate duplication or improve intragovernmental coordination of scientific activities; and 4) suggestions intended to improve and increase the basic research activities of government agencies.

The House Committee on Science and Technology has devoted much of its attention to these kinds of questions for the past five or six years. A pessimistic interpretation is that we have not made much progress in forty years. However, more accurately, I think it points to the enduring nature of a number of larger issues in science policy that will never be settled with finality. In each generation we must deal with these issues by taking those actions which are appropriate to the special circumstances and problems of the times.

Federal Role in Support of Science and Technology

Among the policy issues in a continual state of flux is the federal role in support of science and technology. Some support has been provided since the beginning of our nation; however, the federal role changed dramatically as as result of World War II. And for over twenty-five years -- until the mid-to-late 1960's -- federal support grew rapidly. In varying degrees, R&D became an integral part of virtually every federal agency and came within the province of almost every committee of Congress.

Although the magnitude of support has become large, its nature and emphasis have changed over time. For example:

1. Just prior to 1940, nearly 90 percent of all federally supported research was performed "in house," whereas for Fiscal Year 1976 the percentage for "in house" R&D and management of extramural work is 27 percent. Thus, despite the fears of the 1930's, the federal government has become the major provider of money for R&D for some industries and most university research. I will return later to the question of whether or not this has brought political control.

2. In 1930, nearly 40 percent of all federally supported research was provided by the Department of Agriculture. For 1976, it is 2 percent and there is a growing consensus among the Congress, the Administration, and the scientific community that this is not enough. This consensus has emerged from a debate on the current and long-range prospects for world-wide food and nutrition -- and on the role that science and technology should play. I might add that the House is taking action this week on the "National Agricultural Research Policy Act of 1976" which will place much greater emphasis on agricultural research in relation to world-wide food problems. There is a good chance that this Act will soon become law.

3. In 1960, about 91 percent of the total federal support of R&D went to the defense-aerospace-atomic energy complex. The comparable amount for 1976 is about 72 percent and reflects a greater share of federal support of R&D for such organizations as the Department of Health, Education and Welfare, the Department of Transportation, the Energy Research and Development Administration's non-nuclear programs, the Department of Commerce, the National

Science Foundation, and the Environmental Protection Agency. These trends reflect growing concern and debate over environmental quality; the cost and quality of health care delivery; efficient transportation; the availability of energy; and the serious problems of urban decay and suburban sprawl -- to mention several critical social problems.

The Allocation Problem for R&D

These changes and trends reflect a multitude of *political* decisions, all of which are related to this fundamental problem: how best to determine *who* should spend *how much* R&D money for *what* kinds of purposes. As Willis Shapley points out in Chapter II of his report, one form of the debate on this problem has centered on whether or not there is or should be an "R&D Budget" for the U.S. I agree with his view that there is no "R&D Budget" as such, and that agency heads should be responsible for formulating their budgets -- including such R&D as considered necessary to perform their missions. However, I believe there is a reasonably strong case for going beyond the OMB "cross-cut" analysis of R&D and introducing a stronger central function for analysis at the top. I'll explain why I take this stand.

First, I agree with Harvey Brooks that the root of resource allocation problems is that science and technology really form a single, strongly interacting system that is not well understood, and that most of the theories on planning and allocation proposed in the literature and used in actual decision-making place emphasis on only some aspect of the system, such as basic research or applied technology. As Brooks says, "we have to develop a much more sophisticated understanding of how the existing system works before we can control it." I think this calls for a strong overview -- without giving up pluralism of performance and the majority of individual decisions.

Second, an added dimension of complexity has been noted by Edward Shils, Editor of *Minerva*, who suggests that from the long philosophical debate on the allocation of resources has come the acceptance of two independent and incommensurable criteria of scientific choice: scientific value and practical value. These distinctions correspond roughly to the divergent views of those who argue, respectively, for the autonomy of science and the comprehensive direction of science toward technological applications.

That there is tension and conflict between these differing views has been well established. However, what is new within the past decade or so is increasing presidential and congressional insistence that research and development and their uses be more closely linked. In short, there has been an increasing tendency of many in politics to seek greater and faster "payoffs" from the nation's investment in R&D. In a very real sense, the past successes of scientists and engineers have led to rising expectations on the part of politicians. This takes us back to the point raised earlier that scientists fear political control of science.

Great care is needed as we move forward to assure that we engage in debate with a minimum of acrimony and mutual suspicion. Together, scientists and politicians must struggle to achieve the kind of understanding called for by Brooks. I believe it is as wrong for scientists always to demand complete freedom in conducting research as it is for Presidents and the Congress always to specify in elaborate detail what work shall be done to achieve "quick payoffs" -- particularly for unrealistic political purposes. I think, too, that it is incorrect for the Congress to legislate detailed research programs for specific problems as has been the tradition in agricultural research for a century or more -- unless there is a strongly demonstrated need for such action.

I do not wish to minimize the importance of the federal "control" concern. Federal support of science is an element of the larger issue of federal support of numerous societal functions -- from education to welfare. Political requirements are placed on all institutions and activities receiving federal funds. Probably all institutions -- and state and local governments -- would prefer "no strings" money; but there are other forces which call for accountability in the use of public money. Inevitably, these requirements have some impact on those who receive support from public funds. All of which leads to the need for an intricate balance of interests; but I am optimistic -- solving such problems of balance is the genius of our political system.

In the institutional setting of the Congress, we must guard against the tendencies of some who would interpose Congress into the detailed procedures for selection of research projects and the determination of who should perform them. Yet, it is entirely proper for the Congress to express itself in broad policy matters such as whether or not to embark upon a manned lunar program or the development of a major, multi-billion dollar weapon system. Further, in carrying out its oversight responsibilities the Congress should examine the performance of agencies and render evaluations on management effectiveness and determine whether policy guidance of the Congress is being followed.

Too, I think it is an appropriate function of the Congress to originate and to establish new programs and new organizations when there is an important perceived need. Examples are the "National Agricultural Research Policy Act of 1976," which I mentioned earlier, and the "National Science and Technology Policy, Organization, and Priorities Act of 1976," recently enacted into law. This latter Act takes me to my next point.

A New Organization: The Office of Science and Technology Policy

Moving past the issue of a "total R&D budget," there is the set of policy decisions and problems related to allocation of research and development resources to and for the various departments of government. Then, too, there are other types of more general issues such as scientific and technological transfer and interchange with other nations, such as the Soviet Union, and the relationships of federal R&D with that supported by industry, a prime example being the burgeoning field of energy R&D.

Such decisions and issues have increasingly assumed greater importance at the presidential level. Jerome Wiesner has argued that as federal support and programs have grown, there have been steadily increasing "efforts toward coherence and integration at the presidential level to make more productive and effective use of scarce manpower, facilities and fiscal resources."

Wiesner's point of view is very similar to that adopted by the House Committee on Science and Technology when it began a series of hearings three years ago on "Federal Policy, Plans, and Organization for Science and Technology." These hearings were prompted by the 1973 abolishment of the Office of Science and Technology in the Executive Office and the designation of the Director of the National Science Foundation (NSF) as a part-time Science Advisor. Three years and another President later the position of Science Advisor and a new Office of Science and Technology Policy have been restored to the Executive Office.

We see this new Office as the pragmatic realization that "science permeates all important national issues" -- as Dr. Lewis H. Sarett testified before our Committee just a year ago. Lest there be any who still do not understand this point, let me state it clearly: Office is designed to facilitate the use of contributions of science and technology in national decision processes; it is not to be a spokesman or advocate for science and technology.

Support of Basic Research

There will be occasions when it may appear that the Office is performing the role of science advocate; but even then it will be acting in the context of larger national purposes. An example in the Fiscal Year 1977 budget is useful to illustrate my point: in the final stages of the budget formulation, as I understand it, the President added $50 million for basic research to the NSF's budget based on analyses provided by the Office of Management and Budget (OMB) and Dr. Stever, his Science Advisor.

As his reason, the President referred to the high value placed by "our Founding Fathers" on the pursuit of knowledge and its application and said, "I fully recognize that this country's future -- and that of civilization as well -- depends on nurturing and drawing on the creativity of men and women in our scientific and engineering community. I agree with this view and I have supported the NSF budget request, after making certain adjustments for greater emphasis on science education.

We are not of one mind in the Congress on this increase for basic research, as most of you probably know. I have no wish to criticize my good friends on the House Appropriations Committee for their position in this matter. They believe firmly in their case, as do we in ours. Our differences are a reflection of the great difficulties that face us as a nation in setting "proper" levels for basic research. Our Committee has placed extensive reliance upon the economic fact that, in constant dollars, basic research has decreased since 1967 by about 20 percent. Our Appropriations friends do not believe this is a reliable indicator of decreasing "real" research. This is a difference not easily resolved, and it takes me to my next major observation.

Science Indicators

In May, Representative Ray Thornton's Subcommittee on Domestic and International Scientific Planning and Analysis began hearings on Science Indicators: 1974 that was recently issued by the National Science Board. These hearings arose from our broad concern about how to do a better job of measuring and evaluating the results of federally supported research and development. In the past, science indicators have focused primarily on "inputs," and a noteworthy feature of the new Board report is the inclusion of a number of "output" indicators.

In his testimony, Dr. James H. Zumberge of the National Science Board said that the further development of "output" indicators "will require even more determined and creative efforts, as we attempt to uncover the complex interactions of science, technology and society. These efforts will involve . . . an expansion into previously uncharted areas." These activities of the National Science Board are to be applauded; they are representative of the type of analysis which will be of enormous value to the President and the Congress as we struggle collectively to make better decisions in a world that grows more complex.

Public Attitudes About Science and Technology

Apart from, but related to, the attitudes of those in government and in science is this salient feature of modern society: Public attitudes toward the value of scientific research seem to be changing. For example, Todd La Porte and Daniel Metlay have reported that "supportive of science yet guarded about technology, the public is uneasy about future technological developments." Also, such individuals as Dr. Edward E. David, Jr. believe that public attitudes will influence increasingly the directions and content of scientific and technological programs in the future.

An important example of Dr. David's view is that controversial subjects within the social sciences have aroused emotional debate on the content, validity and ultimate ends of social science research. The development and implementation of science curricula by federal agencies, particularly social science projects, have led to a serious reexamination of such projects in relationship to our philosophy of public education. The recent debate in California over the issue of nuclear power provides another example of growing public interaction with scientific and technological programs.

Taken together, these views and these examples lead me to this conclusion: Ways must be found to provide for an informed electorate to deal with an increasing number of complex problems. The NSF has developed a "Public Understanding of Science" program to enhance science literacy. Our friends in the Senate have proposed in their version of the 1977 NSF Authorization an expansion of a "Science for Citizens" activity that would provide for NSF funding of groups involved with public issues requiring scientific and technological expertise. It is not that we in the House shy away from controversy, nor should the Foundation remain aloof from subjects simply because they are controversial, but we have a genuine concern about whether or not the funding of public advocacy groups is a proper function of the Foundation.

Another concept receiving some public attention, but which has been only briefly considered by the Congress, is the "science court." I understand that it has been discussed by the President's two new advisory groups -- appointed last November as a precursor to the Office of Science and Technology Policy -- in this form: Is it possible to set up a group of scientists or others who actually look at a scientifically controversial subject and try to make statements that could be considered reasonably correct and that would be usable by society? Perhaps my political bias is too evident, but my instinct tells me that science will have its primary effects in the crucible of political debate rather than in judicial chambers.

Indeed, I should like to throw the challenge to you and your organization to engage in the debate on how best to increase public understanding of science and issues having significant scientific and technological content. Perhaps it is a matter that your chapters around the country should add to their active agendas.

Another challenge for you and for those of us in government is to bring about a better understanding of the essential role of industrial research as part of our total national effort. I believe that too little attention is devoted to this aspect of R&D. As Dr. Sarett testified before our Committee last year, "recognition should be given to industry's special role in the effective development of products that serve a useful public purpose.

Conclusion

In pulling together the strands of the ideas which I have talked about today, it is evident that government and science have become closely intertwined. Each affects the other, and while the potential for "control" is present -- of science by government and of government by science -- I do not think such simplistic concerns reflect the realities.

At times, arrogance and stupidity may afflict individual politicians and scientists; at times, petty concerns may rise to the fore; but despite such aberrations, we seem to be devising a system of enduring workability for the benefit of society. And in our complex symbiotic relationship we each must strive to be more appreciative of the subtleties, the world view, and the pressures affecting the work of those in science and of those in government.

In concluding, I have the disturbing feeling that I have brought to you far more problems than solutions today. But if I do not bring specific solutions, let me share with you several principles which are among those at the core of my own beliefs. My old friend and colleague, Charles Mosher, who surely is one of the wisest men ever to grace the halls of Congress, said recently: "I could hardly think of anything more fundamental than the fact that the scientific community must no longer be timid politically in asserting what they or any individual scientist believes is important to the national interest and the interest of mankind. It will take a lot of political courage to do this."

And, finally, in our Bicentennial Year it is most appropriate for politicians and scientists alike to remember Jefferson's declaration that "I have sworn on the altar of God eternal hostility against every form of tyranny over the mind of man."

Discussion

Much of the discussion pertained to the implications for the R&D budget process of the pluralism and diversity that characterize both the practice and funding of science and technology in the United States.

In answer to a direct question, Stever admitted that the idea of a single lead agency for R&D has a certain appeal from the standpoint of cost effectiveness and to provide a focal point for information flow. However, each R&D agency has a different kind of strength, and each contributes a different perspective to the conception and solution of a particular problem. There is a real danger that having a lead agency would destroy that diversity of capability, that there would be no communication with any other agency and, as a consequence, such a lead agency would be cut off from the larger society.

Some concern was voiced about the possibility that the availability of funding by the federal government would eliminate alternate funding sources, particularly private sources and state governments.

The panel of speakers regarded that concern as ill-founded, at least as far as private funding is concerned. Virtually the entire increase in R&D support since the late 1940's has come from government sources, which indicates that there were never any appreciable private funds. In fact, during the last decade industry-supported R&D has increased by 25 percent while federally-supported R&D has decreased by 20 percent. But there is considerable concern among planners at the federal level about the divergence between university-based and industry-based research that has occurred during the past thirty years.

There is evidence, however, that the availability of federal funds has resulted in an overall decrease in the level of constant dollar support for R&D from state governments, including operating support for state universities. O'Neill was asked about the feasibility of using revenue sharing mechanisms to enhance state contributions. He saw no reason why such programs could not be legally instituted under present statutory provisions. O'Neill personally did not favor the idea since revenue sharing implies the existence of a misallocation between the country's institutions and its wealth-producing base. He had yet to be convinced that such a misallocation existed in relation to R&D. A second reason for revenue sharing would be a conviction that state and local governments were deficient in formulating effective public policy and, therefore, the federal government ought to take over. It would be exceedingly difficult to make a politically acceptable case for that assumption.

There were several references throughout the plenary session to the fact that the Office of Management and Budget (OMB), the agencies, and Congress had become much more sophisticated about budgeting for R&D during the past ten or fifteen years. However, it was also suggested that several new types of issues for which past experience may not be a reliable guide are becoming important: For example, the Energy Research and Development Administration (ERDA) has the task of developing technologies that can be used by, and produce a profit for, the private sector, whereas in the past the federal government itself has been the primary consumer of its own R&D. There is also a recent tendency to ask R&D to provide a basis for controlling technology by public participation in decision-making. A whole new set of institutions will probably be required to carry out such a program. Yet OMB seems to have taken a firm stand against granting direct institutional support from the federal government.

Considerable sentiment was expressed about the desirability of granting a preferred status to basic research, based primarily on the argument that guaranteed long-term stability is essential to continuing productivity in basic research.

Stever suggested that occasional crises in the level of basic research support were not necessarily bad. The exponential growth of a particular component of the budget is not in the best interest of either that component or of the larger society. Occasional crises, such as the crisis engendered by the Mansfield amendment, or the energy crisis, exert a quality squeeze and force a reorientation of priorities. Although some areas suffer in such a selection process, the overall, long-range effect of such pruning is almost always positive.

More than one participant suggested that there might be a greater degree of stability if the budget process would explicitly separate the research component from the development component, primarily on the grounds that the time scales for payoffs in the two areas are considerably different. However, the panel was not convinced that there have been such violent or unintentional fluctuations in the research side of the budget as have been suggested. It was also noted that most if not all the major R&D agencies are now making a conscious effort to separate R from D in their budgetary processes. It is true that the agencies may not use identical criteria in separating the two components, or in separating research from other components such as education. This problem is especially severe in the medical research area. The scientific community could be exceedingly helpful to the agencies and to OMB if it would develop rigorous criteria for identifying the basic research components of the agency budgets.

There is considerable concern at present about finding effective means for measuring the outputs from R&D expenditures. The National Science Board has attempted to develop a host of science indicators. However, it is not at all clear that these criteria are in any way superior to traditional semi-intuitive criteria. Obviously, it is very difficult to justify basic research expenditures in terms of quantitative criteria since the whole point of the enterprise is to create knowledge, some of which may prove to be of direct use to society only from a long-term perspective.

The long-term stability issue is strongly coupled with the scientific manpower problem. Many universities are confronted with a decreasing interest in science on the part of undergraduates and an increasing interest in preprofessional areas such as medicine and law. This may be due to a perception by undergraduates that the federal government is not interested in providing support for graduate study in science, as well as their perception that few satisfying careers in basic research will be available to them if and when they complete those studies.

O'Neill stated that the federal government is committed to a policy of funding the first degree above high school level for all qualified students. However, except in the health area, the cost of studying for an advanced degree should be the responsibility of the student, though several modes of federal assistance are available, including the guaranteed student loan program.

Many speakers and participants expressed the hope that the dialogue on the R&D budget initiated by AAAS could be continued and expanded to a point at which the scientific community would be making significant inputs to the budgetary decision-making process. O'Neill noted that if such inputs were to be at all helpful they could not be tantamount to simple requests for increases in R&D funding levels. Rather, they would have to be made within the context of a trade-off or reallocation strategy. A useful exercise for a group of scientists working in a particular disciplinary area would be to study the President's FY 1977 budget, rearrange the allocations within their area, and develop a rationale for those rearrangements. In this way the bases for priority choices would be reexamined. Such exercises would be helpful to OMB since they would be related to the decision-making process that both the President and the Congress are concerned with.

Summary

Ruth M. Davis
Director, Institute for Computer
Sciences & Technology, NBS

I'll now turn the podium over to Dr. Ruth Davis who has graciously agreed to summarize the afternoon's presentations and to record the lessons we've learned here today.

I think my summary, as is the case with most summaries, will prove again that "the whole is usually less than the sum of its parts." In this case, my summary will be far less than the "parts" of the colloquium we have just heard. Nevertheless, I propose to suggest some of the themes that were brought out this afternoon. Before doing that, let me say that these themes seem entirely consistent with a primary objective of the AAAS in arranging this colloquium; namely, to make the traffic flow between the scientific community and the federal management community a two-way flow. However, the phrase "two-way flow" is not really precise enough, because there are ways in which we can communicate in one direction at one time only. In engineering, those types of communication are called half-duplex or simplex. Communicating simultaneously in both directions is what has really been lacking between the scientific community and the federal management community. And in engineering that is called full-duplex communication. So, I would like to think of the objective of the AAAS as making possible simultaneous communication between the federal management community and the scientific community even if such interaction may, at the time, appear a bit confusing.

I believe that we have seen four themes emerge this afternoon. The first focused on the federal budget process and its many steps. The second concerned the individual scientist as he or she interacts with that federal budget process. The third seemed to be related to the scientific community as a whole as it interacts in a "consensus mode" with the budget process. Finally, the fourth theme revolved around the scientific portion of the federal budget process viewed in its largest context.

Let me give you some examples of what I saw happening this afternoon. When we looked at the federal R&D budget process itself, two or three speakers emphasized the real need to eliminate the mystery that now surrounds it. Willis Shapley's report is really a landmark report in that respect. In twenty-five years, I have never seen a report that laid so bare a process that had been previously closed and far too mysterious. I consider that his report gives the scientific community, with the help of the AAAS, the right to treat the federal budget process like the ancients finally treated fire. Initially, we did not understand the process of fire, so we paid blind homage both to it and to its creators. Then, just as when once we understood fire, we used both it and the processes that created it to our advantage, so now that we understand sufficiently the federal budget process, we can also use it to our advantage.

I see no reason why, as scientists and as scientific managers, we cannot interact with the federal budget process now in a very utilitarian way to achieve a number of worthwhile objectives, many of which were touched upon this afternoon.

First, it was suggested, on several occasions, that we treat the federal budget process as an ordinary process and therefore as a basis for interaction. That means knowing when to interact with it and how.

For example, I cannot think of anything more demeaning to a professional than to try to interact honestly, to influence, to try to get a position across, and then to realize that you are too late, that people have not told you you are too late, that you did not know whom to talk to, that you were talking to the wrong people, and that, in fact, you were not being given any of the information you needed to provide your input correctly and properly.

And that is really what has been happening to many of the professionals in the scientific community who have tried to affect the budget and who have not been able to. As Willis' report emphasizes, for example, we have to know that we need to get in there this month in order to affect what's going to happen to our institutions or ourselves in 1978! Since we have a pluralistic budget process and a pluralistic research community, we had better know that we have to get the players in that process in communication with mission-oriented agencies before Christmas of 1976 if we want to affect what is going to happen to us in 1979.

To go in now and say we are concerned about 1976 or 1977 makes people in the federal government feel more than a little unhappy and frustrated and it makes scientists feel more than a little useless since it is now too late to affect the FY 1977 and, in some aspects, the FY 1978 federal budget.

Understanding the R&D budget process also means understanding that we have the right to talk seriously about changes in that process. Several examples of change were mentioned this afternoon. Congress has already changed the process, and Representative Symington noted some other ways that Congress thinks it should be changed. We have heard about the desirability for more centralized planning -- call it higher level cross-cut -- than we have now.

We also need to understand the budget process in order to allow -- and to encourage -- learning. I think Paul O'Neill was quite right in saying, from his own experience as well as from a number of other experiences, that the learning involved in budget-making is not carried forward from one generation of budgeteers to the next. Some excellent learning takes place, but it disappears as budgeteers leave and it gets lost in the mire. There is presently no mechanism in place to transfer that learning and experience to individuals as they first join the ranks of federal manager, scientist or budgeteer. The way to transfer that learning is probably not to try to change the tenure of those in the budget process, because in Washington the most transient phenomena are people, but rather to translate that learning experience to those people who are affected by the budget process. Since we are talking about science, that means translating that experience and learning into the lives of scientists and into the education processes of scientific institutions.

Finally, a number of speakers brought out that we really need to understand the budget process in order to influence and to participate in a peer relationship with federal budget-makers. That cannot be done if "we do not know the territory," as the musical The Music Man so aptly told us. Scientists really have to know their territory. Since the federal budget-making process is carried out within the government, then it is up to the scientific community to learn the territory, as opposed to requiring the federal government to learn the scientific territory.

Let me deal for a few minutes with the second theme -- that of interaction by individual scientists with the federal budget process. It appears, from what we have heard this afternoon, that individual scientists have extremely important roles to play. They, of course, have their own research and their own professional positions to maintain and which they can properly use as vantage points from which they can influence the world productively. But to do so, they must be able to understand the process by which money is distributed. They must understand that that process does not necessarily view scientists as individuals, but simply as small pieces within a much bigger context. Their individual research itself has to be put in a larger setting -- a setting in which priorities have been set, in which they took no part, and in which they previously did not know how to take part. An individual scientist interested in some delightful area of combinatorial mathematics, say, must understand that perhaps combinatorial mathematics is not, as Guy Stever said, changing as rapidly as biological science and space research and that, therefore, little attention is being paid to its advance or support by the government.

There also has to be an awareness on the part of the individual scientist of how to start new projects. That was one of the few noticeable gaps in this afternoon's proceedings. No one told us how to go about getting a new project started in the budget process except to go in and be an adversary or an advocate to existing programs. Indeed, one of the themes that kept cycling through the discussion this afternoon was a mixed one that tied itself to the roles of "adversary" and "advocate." One had the feeling that the general role scientists are expected to play is that of either advocates or adversaries. And yet, I would conjecture that by so doing they can do no greater disservice to themselves. I thought it very interesting that Paul O'Neill emphasized the finite resources available for parcelling out by the Office of Management and Budget (OMB). I had the feeling that I was listening to a description of a process in which someone was trying to stake out a claim as an advocate against an adversary who would dole out a very finite amount of resources based primarily on how the claimant impressed him as an advocate of a particular position.

Whether or not the advocate-adversary process is a proper one, it is certainly one that has not been considered adequately by the scientific community. Representative Symington commented that the simple act of focusing a discussion on science tends to institutionalize an adversary relationship in a community which has had perhaps the least experience of any community in adversary-advocacy relationships.

The third theme I discerned in the discussion this afternoon was the minimum amount of cooperation, structure, and consensus that the scientific community is willing to permit in order to keep itself viable and in order to be a participant in the rather important issues in which science is a very important part.

We have talked about and listened to a discussion of a scientific community that does not appear to be governed so much by the scientific product it produces as by the federal budget process. A great deal was said about accountability by Guy Stever, by Paul O'Neill, and by Representative Symington. We talked far less about the industrial R&D budget process although its "mysterious influence" did pervade the afternoon in subtle ways. But I do not remember anyone bringing up the concept of accountability in the industrial sector as was so frequently mentioned for the governmental sector. Obviously, very little is known about industrial R&D budget processes, although the industrial sector probably has more dollars and more people involved in R&D than the entire federal government.

The total national set of budget processes seems to exert more influence on the activities of the scientific community than do its research objectives or products. Presently, there exists a delightful chaos regarding the relationships between state and local government and the federal government, and between these potential sources of funding for scientific research -- and any others you care to name -- and the scientific community.

So it seems that today we have a scientific community that has just begun asking itself what minimum amount of work is it willing to do as a community when it is governed by at least three budget processes with which it must interact in order to get its priorities and objectives translated into budgetary actions.

Finally, we heard a very important fourth theme, time and time again; namely, that no matter how we look at "science" -- as a community, as a product, as a set of individuals, or as a part of a budget process -- it really has become increasingly important on the international scene. As Guy Stever correctly noted, science has become an international commodity. We barter it in a diplomatic marketplace, we barter it in the marketplace of economic competition between nations, and we barter it in the battlefield of national defense. We also barter in terms of our own safety, as in the case of nuclear plants, for example. Most, if not all, of the fifty states are looking at the problems of the technical aspects of privacy as highlighted by the advent of the computer age. In other words, science has become a political handball.

Simultaneously, the educational community is concerned about having either too many or too few students, their right to choose their professions, and their right to some guarantee of being able to follow their chosen professions when they leave academia.

Finally, the very important political issue of science and the public was brought up by Guy Stever, Paul O'Neill, and Paul Doty. The scientific community now must decide what minimum amount of interaction it is willing to engage in with a lay public that is getting smarter and making more demands about its right to control its own destiny. That lay public believes in participatory democracy, and can see no reason why science is not included in that type of participatory democracy.

In summary, the themes, the context, and the insights we got from the speakers during the three and one-half hours this afternoon were really remarkable. I would like to emphasize once again that the AAAS wants this colloquium to be a starting point, not an end point. As Bill Carey has said, this colloquium is not a fluke, it did not just happen. Rather it is a first signal flare sent up by the AAAS to let the world know that it is one scientific association that wants the scientific community to be more active, to engage in genuine two-way communication, and to be a peer in influencing all kinds of policies. The AAAS heartily encourages not only those of you who are scientists, but those of you who are here from other communities to join with us to help the government -- and all of us who believe in participatory democracy -- to make what is important happen, and happen in time to help all of us.

Thank you very much for your participation.

As usual, you've done an excellent job, Ruth, that we all appreciate. Let me say once again how grateful we are for the time and effort that all of our speakers have given to this session. It is my impression that this colloquium is living up to the expectations of those of us who conceived it.

Workshop 1

BUDGETING FOR BASIC RESEARCH

Participants

Presiding: **Jurgen Schmandt,** *Professor of Public Affairs, LBJ School of Public Affairs, University of Texas*

Rapporteur: **Pamela Ebert,** *Executive Secretary, National Research Council Study on National Research Awards*

Prepared Presentations: **Philip M. Smith,** *Special Assistant to the Director, National Science Foundation;** **Ronald W. Lamont-Havers,** *Deputy Director, National Institutes of Health;* **Daniel R. Miller,** *Deputy Director, Division of Physical Research, Energy Research and Development Administration*

Invited Comments: **William G. Wells,** *Technical Consultant, House Committee on Science and Technology;* **Ronald M. Konkel,** *Chief, Science and Space Programs Branch, Office of Management and Budget;* **John Allen,** *Deputy Director, Research and Advanced Technology, Department of Defense;* **Clifford Grobstein,** *Professor of Biology and Vice Chancellor, University of California, San Diego;* **W. Richard Werthamer,** *Chairman, New York State Energy Research and Development Authority*

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*Philip M. Smith is now at the Office of Science and Technology Policy, Executive Office of the President

Budgeting for Basic Research

Introduction

Jurgen Schmandt opened the workshop by noting that approximately 10 percent of the $24 billion for R&D in the President's FY 1977 budget is earmarked for basic research. However, this same budget may be analyzed with respect to: the relevance of areas of support, the effectiveness of outputs, and the degree of continuity and stability in the face of inflation. At least six federal agencies now provide the majority of support for basic research. At least four authorization subcommittees on each of the two sides of Congress, and an equal number of appropriations subcommittees, are concerned with the budgets for these agencies. Therefore, in attempting to understand the budget process, it is important to identify the people who make the decisions regarding basic research support and to ask what criteria they use to make those decisions.

Prepared Presentations

Philip M. Smith stated that there is by now a reasonable level of understanding in the Office of Management and Budget and on Capitol Hill about what basic research is, as well as a recognition that there should be federal support for basic research. However, the need for constant reeducation, for explaining to decisionmakers what changes are taking place within and among the various disciplines is likely to remain. Most people in the political arena also have a hard time handling the fifteen, twenty, or twenty-five year payoff periods for basic research that scientists are accustomed to.

The infrastructure for basic research in the United States is basically sound, despite several important institutional problems that include increasing direct and indirect costs, and aging university faculties. However, these problems, coupled with the growing sophistication about basic research on Capitol Hill and the rising demand for public accountability, challenge the collective scientific community to help develop more finely-tuned techniques for deciding how and where to make basic research investments.

In the past, the National Science Foundation has used a number of ploys and techniques to rationalize increases in its basic research budget requests. For example, this past year NSF was able to convince OMB that, since basic research budget increases had not kept up with inflation during the past three or four years, an additional $50 million increase would be appropriate. Obviously that argument is unlikely to be effective for FY 1978 or any other near-term budget year.

NSF has frequently used one-time capital investments in a particular basic research discipline as a focus of budgetary concern. That approach can be effective in the short term, however its long-term consequences have not been addressed adequately. New programs added in this way become part of the base. Over the years NSF has tended to accumulate programs that add to the base. In the future we who support science will have to consider using differential investment patterns rather than automatic incremental methods to respond to needs articulated at the program level.

Ronald Lamont-Havers stressed that the National Institutes of Health should be regarded as a health agency rather than a research agency. The federal government has supported biomedical research within NIH's eleven component institutes because society perceives that such research will contribute to better health for the American public.

NIH is well aware that the support which it receives from the Administration and the Congress is largely a reflection and result of the interest of the populace in its programs. The manner in which its budget is developed therefore must be viewed in light of social pressures brought to bear by groups interested in specific categorical or directed areas. In making decisions about research investments, primary consideration is given to the efforts that both the Administration and Congress agree have the highest priority.* At present these programs are cancer and heart disease. In addition, NIH itself has identified certain areas, such as environmental sciences and immunology, as needing "protection" and/or special stimulation.

NIH prefers to use the term non-categorical research rather than basic research in developing its budgets. Its mission is to better the health of mankind and, from that perspective, all NIH research should be categorized as applied. However, approximately 30 percent of the research supported by NIH can be regarded as non-categorical, or basic. During the next decade the pressures on NIH to interface with health care delivery systems is bound to increase, leading to a situation in which support for non-categorical research could disappear unless care is taken.

There has been an increasing interest of late in NIH's role in technology assessment and technology transfer. During the next decade a primary concern will be to refine techniques so that technology assessment and transfer can be effectively accomplished without diminishing the initiative or spontaneity of discovery. This interest of Congress and the public does not indicate a disenchantment with biomedical research, but rather suggests the need to question whether the coordination of such research is as effective as it might be.

Daniel Miller began by reminding the audience that the Energy Research and Development Administration was just established in January 1975. He felt that several issues of particular concern to the agency as it confronts the budget process are likely to become increasingly important to basic research in general.

For example, any vigorous and productive field of basic science must be concerned not only with what it should do now, but also with where it should go in the future. It is necessary to ask, then, what fraction of the total resources in a field should be devoted to utilizing existing research facilities, and what fraction devoted to fabricating facilities to provide completely new capabilities for the field. The question is particularly important for the big-science fields, such as high-energy physics, in which 20 to 25 percent of the budgets over the past fifteen years have been devoted to the construction of new facilities.

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*R&D budget breakdowns for the component institutes of NIH are given in the Shapley report, p. 76.

The desirability of assuring stability and continuity in the level of support for various basic research fields has frequently been stressed. However, arguments in favor of guaranteeing long-term levels of support often reduce to assertions that basic research is different from other federally-supported programs. It is not at all clear that a case for a preferred status has been made or can be made. It may be more appropriate to develop a rationale for stability in terms of the fundamental mismatch between the time scale of the annual federal budget cycle and the time scale on which basic research is carried out and its facilities planned and constructed. For example, in high-energy physics the typical time interval between the initial submittal of an experimental proposal to an accelerator installation and publication of the research results is about three years, while the time elapsed between a formal proposal for a major new facility and its first use for research purposes is about ten years. The European Center for Nuclear Research at Geneva (CERN) has developed a four-year budget cycle. However, the mismatch between time scales is likely to remain a problem in the United States, although realistic long-range planning can help to reduce or eliminate some of the adverse effects of that mismatch.

Invited Comments

William G. Wells, Jr. began by amplifying a remark made by Representative Symington at the opening plenary session that analyses of the R&D portions of the federal budget are needed which go beyond the "cross-cut" offered by the Office of Management and Budget in its special analyses. Wells suggested that Symington had two things in mind:

1) The expectation that the new White House Office of Science and Technology Policy will provide the sort of continuing, broad-guaged analysis required for a better understanding of the entire spectrum of science and technology, and

2) The hope that appropriate congressional committees, including the new budget committees, will also develop more sophisticated approaches to the R&D budget process. Wells said that the House Committee on Science and Technology has no very refined tools for assessing R&D budgets. This year its judgments on the NSF budget were based on many staff hours spent talking with NSF program officers, consultations with people at OMB, and review of budget and oversight hearing records. Ultimately, the committee agreed that the proposed $50 million increase referred to earlier by Smith was justified. The House Appropriations Committee disagreed with this judgment, asking why NSF should be singled out to catch up with inflation when inflation plagues every government agency and every sector of society. Since no adequate tools exist for measuring basic research outputs, that question can only be answered by intuitive arguments about the need for a healthy basic research enterprise. It is hoped the Office of Science and Technology Policy will be able to advance the development of some of the analytic tools required to make quantitative judgments on a range of issues related to the R&D budgets. An example is the work being sponsored by the National Science Board on "output indicators."

Wells also suggested that, since science and scientists had become increasingly active participants in the political process since the mid-1930's, a greater degree of political sophistication was required in the scientific community. Lobbying is a respectable and necessary function in our governmental system and major "interests" over the years have developed skillful techniques in advancing their views. Yet such lobbying as is done by the scientific community tends to be intermittent, ineffectual, and amateurish. Too often there is expressed the view: "But society owes us support -- for its own good." In short, much more attention must be given to the study of science in a political setting -- and to how the political process can be affected.

Ronald M. Konkel agreed with Schmandt and Smith about the general acceptance of the federal government's role as the primary supporter of basic research. However, OMB has some concerns about the productivity of the basic research effort as well as the fact that no one knows how to measure that productivity very well. A good case might be made that basic research funding should be considered on an incremental basis because it is so difficult to assess how much is enough. Although an incremental approach makes it difficult to argue for annual increases, it does provide a measure of long-term stability.

Konkel raised two questions regarding Smith's suggestion on the desirability of a differential investment strategy for basic research. First, does it make sense even to think in terms of a national investment policy with respect to basic research areas? If so, how would such a strategy be implemented? Certainly the budget process could be comfortable with a differential investment strategy if a meaningful strategy were defined. However, a broad consensus would be required regarding which research areas were to be encouraged at the expense of others whose level of support would necessarily be decreased.

John Allen noted that since the approximately $400 million research budget in the Department of Defense (DOD) is a small fraction of its $100 billion plus budget, it is not scrutinized intensively by Congress. Congress has forbidden DOD to engage in certain specific activities such as research related to climate modification and research funding justified principally on the grounds of the support of education. However, by and large, funding levels for research are determined internally. Two years ago DOD decided to reverse the long-term decline in its research support. It also decided to reduce its in-house research from 60 percent of the total to 45 percent, and it is now actively soliciting research proposals.

Allen agreed with the previous speakers that decisions on appropriations are usually intuitive. He had yet to see any output indicators study that could provide definitive criteria for judging the value of a particular research program, and preferred to rely instead on a peer review process.

Clifford Grobstein emphasized that basic research should be regarded as one essential component in a much larger, continuous system that has to do with the flow of knowledge. Basic research contributes to that larger system and is influenced by it. Likewise, it interacts with and is influenced by social needs.

Despite its continuity with a larger system, it is necessary to develop means for insuring that basic research receives appropriate support consistent with its contributions to the larger total system. Basic research differs in its sociology from the other components of that system. For example, most basic research is done at research universities by Ph.D's. Thus, it interacts not only with the training of graduate students in scientific disciplines, but also with the training of all students, particularly professional students, in an increasingly knowledge-intensive society. Since basic research has important, indirect impacts on other areas, it is important to look at it separately; not to budget for basic research separately, but to evaluate the budgeting process in terms of all of its direct and indirect impacts.

N. Richard Werthamer stated that a major source of personal concern is the decline in basic research investments by technological industrial firms. At the same time university-based scientists in the United States are deeply worried about finding jobs for Ph.D. students. A federal grant for basic research at a university usually provides support for graduate students working in that research program. Consequently, grant and contract funds have led to the production of students for the saturated university job market.

Werthamer suggested that federal funding agencies consider putting a larger fraction of their grant funds into basic research in industry rather than into the universities on the condition that those industrial firms which receive grants divert some of the money to universities in their own geographic regions. In this way research performed at the universities would become more closely attuned to industrial needs, and the students who become involved in that research could prepare themselves for available jobs in industry.

Discussion

The discussion period opened with a question about how scientists, particularly in the universities, can have a meaningful input into budgetary decision-making processes. Smith responded by noting the evidence of growing interest on the part of professional scientific societies regarding actions by Congress on the NSF budget.

Wells agreed that the efforts made by scientists this year on behalf of the FY 1977 NSF budget have made a difference on Capitol Hill. He suggested two mechanisms through which the scientific community could exert a continuing influence on Congress. First, groups of scientists in specific congressional districts (or states) could offer their services as informal advisory committees to their representative (or senator). Second, the scientific community could organize a lobbying group with a paid professional staff to represent its interests in Washington.

It was also noted that the budget process is somewhat different in the new "open" government. Congress knows much more about what goes on between the agencies and OMB. And the Freedom of Information Act and the establishment of OSTP have given the agencies themselves a greater role in planning related to budgeting.

Although increasing numbers of scientists have been going to Capitol Hill to urge increases in basic research funding, their approach has been somewhat unsophisticated. In some quarters, according to Smith, it is called a "tin cup" approach. If scientists are to make any significant, sustained impacts on the budget process they must begin thinking in terms of trade-offs and be prepared to talk in terms of alternatives. If a request is made to add something significant in one area, it is highly desirable to offer to give up something elsewhere.

Obviously a trade-off approach could only be effective if there were a measure of consensus about long-range research priorities among the various sectors of the basic research community. The agencies and OMB might be pleased if the scientific community were to identify several candidates for sharply decreasing support, though no group of scientists has ever stepped forward to volunteer such a list. During the next decade the scientific community will have to come to grips with the tough questions of research priorities. For example, Lamont-Havers mentioned that there is bound to be a leveling off of the fraction of the gross national product devoted to biomedical research. The selection of research priority areas also will have an important impact on how many people should be trained and what they should be trained for.

The trade-off issue should be viewed in a trans-agency context. For example, as Smith mentioned, what should be the relationship between the NSF and NASA programs in astronomy? Likewise, what happens to disciplines such as botany or radio astronomy that may be caught in a gap between agencies? Hopefully OSTP will be able to take a broad, long-range view of the basic research enterprise and bring about better inter-agency coordination.

Rational long-range planning for basic research may indeed require the adoption of a trade-off strategy if it is assumed that there is a ceiling to the level of support for basic research. Since no single category of the federal budget is explicitly referred to as the R&D component, in principle there need be no ceiling. However, since the several research and development components compete not only with one another but also with the non-R&D components of the budget in the general framework of the economy, it is unrealistic to expect that basic research will be singled out for preferred treatment.

Reference was made to the problem of identifying suitable criteria for decision-making on support levels for basic research. Derek de Solla Price suggested that such a criterion might be developed by arguing that the value of basic research to society is entirely extrinsic since its practioners perform otherwise valuable social services such as teaching, working in agricultural extension centers, or delivering health care. Society has a commitment to support them against the obsolescence in their value that would be caused if they did not keep abreast of modern knowledge. Through grants and contracts the funding agencies have artificially subsidized the university industry. An extrinsic-merit approach would permit them to subsidize universities directly and explicitly.

It was the consensus of the participants that the scientific community ought to become more deeply involved in the R&D budget process. The AAAS, as a non-government organization, can provide an environment in which interested parties inside the government can interact with members of the research communities. Such continuing interactions have a good chance of being beneficial to all parties.

In summing up the debate, Schmandt made the following points:

There was general agreement that the federal government needs to provide adequate and stable support for basic research. Not to do so would endanger the health of the entire American R&D system.

Little guidance is available on what would be an appropriate support level for basic research. Attempts to establish allocation criteria are useful only to the extent of consideration of scientific criteria. It is as impossible today as it was in the past to measure the economic or social benefits derived from investment in basic research. Given this situation, any effort aimed at predicting possible outcomes is impossible.

Cutbacks in industrial funding of basic research as well as losses in federal support due to lower annual increases and inflationary impact have resulted in reduced training and job opportunities. These developments over time will have a direct impact on the status of American basic science. Given long lead times, it must be realized that fluctuations in basic research funding will have dangerous consequences though they may take a long time to become visible.

While the National Science Foundation and the National Institutes of Health provide major support for basic research, mission-oriented agencies with important research programs should make deliberate efforts in supporting basic research related to their mission objectives.

Workshop 2

BUDGETING FOR DEVELOPMENT AND DEMONSTRATION

Participants

Presiding: Don E. Kash, *Director, Science and Public Policy Program, University of Oklahoma*

Rapporteur: Christopher L. Coccio, *ASME Congressional Engineering Fellow with the Senate Committee on Interior and Insular Affairs*

Prepared Presentations: Ernest S. Chaput, *Director, Budget Programs, Office of the Comptroller, Energy Research and Development Administration;* G.R. Makepeace, *Assistant Director, Defense Research and Engineering, Department of Defense;* Thomas Glennan, *National Academy of Sciences*

Invited Comments: Jon Veigel, *Director, Research and Development, California State Energy Resources Conservation and Development Commission;* Walter Baer, *The RAND Corporation;* Sumner Myers, *Institute of Public Administration*

Budgeting for Development and Demonstration

Introduction

Don Kash opened the workshop by pointing out that more emphasis is now given to the demonstration element in research, development, and demonstration. He argued that the concept of R&D is moving away from the once predominant idea that if technologies were pushed to the point of technological feasibility, they would automatically be picked up and used by the system.

The Energy Research and Development Administration (ERDA) has been established for the express purpose of developing processes and technologies to be used in the private sector to produce energy and, presumably, private profits. There are major differences, then, in the way the public and private sectors will have to work together, and there are many patent and profit problems still to be resolved.

Although the Department of Defense (DOD) both pays for and uses R&D, it has become increasingly evident that, as R&D becomes more complex, those who pay for R&D in DOD also must sell it to the program users.

Finally, he noted that the National Academy of Sciences (NAS) study on demonstrations in the social sector shows that there are very few incentives to use new social techniques, except those incentives built in by the federal government. This is a particularly difficult problem since at present there seems to be, at the state and local levels of government, a growing resistance to taking the lead from the federal government.

Prepared Presentations

Ernest S. Chaput began by outlining the techniques and considerations ERDA employs in reaching budget decisions on research, development, and demonstration, with emphasis on the types of judgments associated with the budget process. He hoped that, by concentrating on process, he could help the scientific community understand how it can meet more effectively the real need for unbiased, scientific input into the decision-making process on programs of national need.

Chaput noted that the Shapley report stresses the role of the budget as an expression of political philosophy based on a perception of national needs. Disagreements over budget content are not necessarily based on the technical merits of programs, but can result from different political approaches to a perceived national need.

In ERDA the planning and budgeting processes are separate. The planning process concentrates on broad program issues and is a blueprint of milestones and objectives for a period of years. In contrast, the budget is a detailed implementation of a one-year slice of that plan.

ERDA is unique in that its ultimate mission is to plan for activities that are intended for eventual commercialization by the private sector. This mission profoundly effects the planning and budgeting processes by forcing decisions on the proper federal role, if any, and the most effective form of assistance. Because commercialization is an ultimate objective, ERDA is committed to fund and manage projects jointly with the private sector. In doing this, ERDA accrues at least three benefits: access to technical and managerial expertise, information gained from private sector decisions on capital allocation for use in evaluation of competing technologies, and timely technology transfer. By working in a commercial framework ERDA can also learn a good deal about market penetration, i.e., how to evaluate the potential for commercial as well as technical and economic success.

A National Plan for Energy Research, Development and*

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*Energy Research and Development Administration. A National Plan for Energy Research, Development & Demonstration: Creating Energy Choices for the Future, 1976. 2 vols.

Demonstration represents ERDA's perception of the federal government's role in finding a solution to the national energy problem. Volume I discusses the scope and ramifications of the problem, and provides technology goals and R&D priorities. Volume II presents the specific activities and milestones, and provides the set of program assumptions to be used as the basis for developing an R&D budget. Both the Office of Management and Budget (OMB) and the Congress recognize the strong relationship between ERDA's planning documents and its budget and, accordingly, review each with the same tenacity and from the same perspective. However, the program pace and mix in the plan are set by technical considerations, while the pace and mix in the budget are additionally influenced by budgetary limitations.

Chaput pointed out that to determine funding priorities for competing programs in its plan, ERDA considers: 1) the requirement for federal funding assistance, 2) the significance of the technology to the overall energy solution, 3) whether the technology will contribute to near-term or long-term solutions, 4) the degree of program maturity, 5) the status of the technology, 6) the need to pursue technical options, and 7) whether the proposed budget is the minimum necessary to attain the objectives.

ERDA's planning and budget proposals are developed collectively by the deputy administrator and programmatic assistant administrators, with help from appropriate staff and inputs from a Program Planning Committee and a Budget Review Committee. Because the plan represents the view of the federal government, it is also reviewed by organizations outside of ERDA. By involving the managers of all ERDA programs in decisions on individual programs, the agency tries to achieve a balance among competing technologies.

Chaput reviewed briefly the increase in ERDA's budget for FY 1977, stressing that levels of funding are not accurate portrayals of commitment to programs, but rather reflect the maturity of the various technologies. In closing, he again emphasized that the value of ERDA's research cannot be solely judged by its technical accomplishments, but that the successful demonstration of the commercial viability of an energy supply or conservation technology must also be considered.

G. R. Makepeace noted that although DOD can only fund technology with direct military use, its range of interest is so wide that a great deal of DOD research, development, test and evaluation (RDT&E) does transfer to non-DOD use. Some examples are aircraft design and computer technology. Although DOD's R&D efforts will undoubtedly continue to have important impacts on the civilian sector, the relative importance of that R&D to the national economy might well diminish because of growing inputs from other agencies, particularly ERDA.

Makepeace said that the translation of new technology to engineering application has always been a difficult process, but today it is more difficult than ever because of the high risk, both in dollars and time, involved in the utilization of new technology. Program managers are reluctant to take risks unless they are unavoidable, or unless programs are amply funded to explore alternatives. Recent pressures on DOD have made risk-taking unpalatable, and funds for alternatives are scarce. Therefore, technology demonstration is even more important now than in the past, though some DOD advisors believe Congress and the Executive Branch must return to a philosophy that accepts difficulties and failures in advanced technology.

DOD's approach to technology demonstration focuses primarily on a number of specific programs aimed at potential end-use applications and continuing demonstration of the realistic use of new components as they emerge from research programs. In a sense, it has adopted an engineering demand pull strategy that is probably the most desirable transfer mechanism available. Two examples of rather specific kinds of demonstrations for a single end-purpose use are the Navy low-volume ram jet where at least three years probably have been saved for a potential urgent application, and work satisfying the need for IR imaging systems at affordable prices, an essential demonstration performed at a relatively low cost.

Two examples from the second type of technology demonstration that DOD practices, i.e., demonstration of breakthroughs or advances as they emerge from the laboratories, are the advanced technology engine gas generator, to demonstrate advanced aircraft engine components and developments in a test engine, and work beginning on the reliability and standardization needed for electron devices transition.

Another approach to technology demonstration, referred to as barrier crossing, can be seen in the area of forecasting ocean wave spectra. Capabilities in this area were developed in the technology phase, then lay dormant for ten years until a complex program was generated specifically to apply the technology to operational use and development. This work has produced a very useful wave climatology. Other significant examples of the barrier crossing approach are rare.

Perhaps the most significant problem DOD faces, in terms of actually implementing its technology demonstration programs, is recognizing real needs before they become obvious. This requires the acquisition and maintenance of funding support. However, technology demonstration programs must compete for support with other pressing needs, including overall protection of technology base funding, and major program dollar adjustments. When a dollar adjustment is needed in a major development program, demonstration programs are visible targets for budget cuts. It is often difficult to secure the attention of a program officer at the development level before engineering designs are too far committed to use a new technology. Phasing and arranging a new technology so that it gets used for a specific end-purpose when it's needed also present problems.

DOD must face the almost certain accusation that any technology demonstration program it performs is too little, too late, and wrongly directed. DOD does not regard its sponsorship of technology demonstrations as a complete, final or wholly satisfactory answer to problems inherent in crossing technology barriers. However, it can be regarded as a necessary and workable tool.

Thomas Glennan said that he would focus on a different activity from a different perspective. NAS has undertaken an examination of social R&D. Social R&D is widely distributed across the government, but none of the agencies where it is found are large, compared to ERDA and DOD, and most have limited management experience with R&D. Many activities classified as R&D are performed in a mission environment or an an operating program environment.

NAS includes in its definition of social R&D all those activities associated with the understanding or alleviation of social problems, and the creation and application of knowledge to deal with those problems. Activities that are primarily technological are excluded for NAS purposes. Demonstration and experimentation, which often do not differ significantly, comprise nearly 22 percent of the social R&D funds, and the process by which they are conducted is not well understood.

Demonstration programs range widely in character. There are projects where intensive program innovation activities take place with the idea that they eventually will be funded for wide diffusion. These are supported largely by agencies specializing in R&D. Other demonstration projects identify existing exemplary activities and support their refinement for use as models to aid the diffusion process. Then there are comparatively large-scale programs that, though they are called demonstrations, really exist to provide services to a limited proportion of a target population. Finally, there are projects that can be likened to pilot tests in which concepts are usually tested in one site. These can be viewed as one stage in a process, much like the more technological developments, but they rarely continue to the next stage.

To help in scanning these activities, NAS has established the following set of descriptors:

1. Policy formulation demonstrations: intended to help federal policy-makers make decisions concerning the worth of a program design for subsequent implementation.

2. Policy implementing demonstrations: intended to help local decision-makers innovate, refine, and reform their activities.

3. Miscellaneous programs not associated with creating or promoting the application of knowledge: i.e., limited service programs that probably are not social R&D, but represent political compromises between those who wish to direct an effort at certain problems and those who do not.

In the early days of the Great Society there were what Glennan called advocacy programs, begun with the notion that by creating centers of activity, attention would be focused on specific social problems. There is also an interesting set of social R&D activities that he referred to as subsidization of local development where limited funds are provided to a local social service with the hope that it will result in development of a new program concept at the local level.

Program objectives often differ according to whom one asks about them. NAS tried to use the federal program managers' intent in every case to categorize programs. Difference of intent constitutes a major problem in the conduct of much social R&D, since every program begins with a compromise among groups having differing intents -- a compromise that often splinters during the course of a project.

A distinguishing characteristic of social demonstration is that there are no dominant technologies to structure implementation. Furthermore, in most of these activities there are no intermediate marketing sectors to provide technical assistance and field distribution. Instead, a direct relationship exists between the contractors and the final users. Communication transfer is also a problem because the users of demonstration projects are extremely diverse both in their capabilities to perform R&D activities and in their geographic distribution. When it is recognized that these activities occur in the public sector, with the attendant political pressures, it becomes apparent just how difficult the problem of translation from an initial concept to a locally implemented program really is. Social demonstration is frequently the vehicle by which the federal government tries to press its priorities on state and local governments, or local service agencies, by providing information necessary to change the behavior of local service delivery operations. It is rarely acknowledged that incentive structures within local social service sectors are different from those of federal decision-makers. Relatively little attention is given to developing standard processes, and relatively little research is done on demonstrations as a policy for social change.

Glennan outlined several improvements he thought necessary.

Each agency should have a clearer sense of the functions of demonstration programs. Often there are different perceptions of the same program. This is due in part to the political nature of many of the demonstrations -- they are not initiated in an orderly R&D process.

There is a seductive quality to demonstration programs, an excitement associated with providing actual services. This frequently leads to a situation in which demonstrations become ends in themselves, rather than the means to effect social policy. Again, this problem emphasizes the need to get a clear sense about program functions.

A better understanding of the market into which these demonstrations will be introduced is also needed. Here Glennan referred to policy implementing demonstrations and said often demonstrations performed require personnel qualities and levels of resources that are unavailable at the local level.

In all these activities there must be local adaptation, rather than mere adoption of a single, well-developed, new technology. Therefore, an understanding of how to balance functions between local and federal planning is needed.

Finally, since local agencies differ a great deal in their capacities, programs must be flexible enough to allow local agencies to implement project designs differently.

Glennan concluded with the following points: 1) Demonstrations are a large and important part of social R&D. 2) Demonstrations are poorly conceptualized as an R&D tool in the social area. 3) While, in many instances, demonstrations are created as a result of political activities external to the R&D system, they represent R&D opportunities that should be used effectively. 4) An important part of the future work of social R&D programs will be the development of a better understanding of the demonstration process, including market analyses and the ways policymakers use knowledge concerning social problems.

Invited Comments

Jon Veigel commented that any discussion of demonstration should include the realization that, in principle, demonstrations are intended to serve a general purpose and may well become policies. He stressed the importance of being aware of the consequences of such policies.

Veigel thought the focus of the presentations had been government involvement in a spectrum of demonstration possibilities -- a continuum that is institutional at one end and individual at the other. The institutional end is best exemplified by DOD where the purchaser of the research is also the purchaser of the product. The other end of the spectrum involves people on an individual basis. Obviously the number of decision-makers needed to approve each step will increase in moving from one end of the continuum to the other. Veigel saw four distinct stages for decision-making on any demonstration, all in terms of options explored and eventually exercised.

The first stage is an option protection phase where there is an idea that an option might work, but neither any assurance that it will, nor any firm knowledge about how to decide to use it if it does. Therefore, the option is protected long enough to discover its quality and the circumstances that will affect its use. If the project stops there, it is usually due to an inherent flaw.

Surviving this stage, an option next enters competition with options that have reached the same level. Here the universe of factors considered in decisions to continue with one option or choose another widen. There is concern not only with the technological and sociological viability of an option, but also with the reaction its appearance will cause in the environment. It would seem that a deliberate choice to continue or not must be made at this stage, but that may not happen unless it is forced, perhaps by limited resources.

Next comes the implementation stage. Once it has been decided that an option or selection of options will work and is economically viable, a different class of problems must be faced. The impact of a single product on a single environment is not considered, but rather the impact of the nearly simultaneous deployment of a variety of options or technologies. Again, there may be resource limitations.

The final sustained growth stage is distinctly different. Somehow the system being implemented has developed sufficient momentum that it is nearly impossible to stop.

In closing, Veigel stated that perhaps the greatest problem is in coping with success of demonstrations. People who are involved in carrying out demonstrations and development may be the wrong ones to decide on deployment and implementation. He suggested that probably more programs should be stopped than actually are stopped.

Walter Baer commented on three points: 1) the overall scale of federally supported demonstrations; 2) who the target audiences for demonstrations are; and 3) the fuzzy boundaries in the budget process between demonstrations and subsequent forms of federal subsidies or support programs.

On the first question, the Rand Corporation recently completed a study of demonstrations supported by federal agencies to stimulate technological innovation. In FY 1974 the level of support for technological demonstration programs was approximately $600-700 million. Demonstration programs are now a billion-dollar business and are likely to increase substantially in the future, particularly in the energy field.

It is important to know who is supposed to make use of a demonstration's results, since this has significant budget implications. For a closed system in which demonstrations are conducted for use by federal agencies themselves (e.g., in space and defense), one can consider budgeting for both demonstration and subsequent implementation. But in programs where the adopter will be a state or local public agency (e.g., in law enforcement and transportation), budgeting becomes more complicated. In the past, there has been too little coupling between demonstrations and subsequent capital grants. The third and largest category are federally funded demonstrations of technologies that are to be picked up by the private sector, as in the energy field. There is yet another kind, not previously mentioned, that will become increasingly important, i.e., demonstrations conducted by federal regulatory agencies to determine what regulations are appropriate. These demonstrations can be particularly useful since they are intended to resolve uncertainties about externalities that are not well determined in the marketplace (e.g., pollution), and about technologies that the private sector cannot be expected to adopt voluntarily.

Finally, Baer commented on the problems of distinguishing between budgeting demonstration projects to provide information, and subsequent subsidy or support programs. In some cases, the support is built in from the program's conception. However, there are other cases where programs begin as one-shot demonstrations, but then seem to require continuing government subsidy. Of course, it is easier to get into things than to get out of them. Agencies may have to buy their way out when demonstrations or R&D projects get into trouble, just as industry often does. The problem for the budget process is how to deal with a need for continuing, large-scale support programs once the demonstration phase has ended. Budget discussions rarely include considerations of the fiscal implications of follow-on activities. But it is usually hard to determine before the demonstration the point at which the federal government can get out of an activity and the private or local sectors can take over.

Sumner Myers titled his comments "Demonstrations Through the Jaundiced Eye." He noted that the term "demonstration" is ambiguous, used to mean both experiment and example.

He focused on demonstrations used as examples to convince industry to pick up and exploit technology, i.e., demonstrations in the context of commercialization. In that context demonstrations are inputs to government agencies' diffusions systems. We have learned that paying for input is very expensive. Perhaps we should pay for output rather than input. For example, the Russians pay a diffusion bonus to the developer of a technology every time an industry picks up and uses that technology. Perhaps a similar system could be adopted in the U.S. for non-monopoly, non-patentable technologies. However, this might be too specific an example of how to solve the problem and reduce the budget. Indeed, it is not clear how well the Russian system works.

Myers suggested the following approach to budgeting for demonstration projects. First, the government should bear the burden of proof for sponsoring a full-scale demonstration intended to convince industry that a technology has economic promise. If industry can be convinced by other, less expensive means, the government should try those first. Failing that, the government should seriously weigh the advantages of spending large amounts of money on a demonstration aimed to convince industry if industry has indicated it is hostile or lukewarm on a particular project.

If an agency chooses to undertake a demonstration, it should manage it, not overmanage it. An exemplary demonstration intended to convince industry should have highly predictable outcomes. Thus the agency can pay for results rather than intentions, and their involvement can be minimal, focusing mainly on contractual arrangements. In short, an agency that governs least governs best.

Our dependence on demonstrations shows a tendency to depend more heavily on technology push than demand pull. In the private sector most innovations occur in response to demand pull. Demand pull for new technologies can be generated by changing the economic environment through various incentive schemes. This should reduce budget requirements, although the economic side effects have yet to be explored. Some incentive approaches require an outlay of federal funds and some do not.

Myers concluded with the general recommendation that incentives should be used to encourage private industry to develop and diffuse its own technology whenever possible, and the burden of proof should be on doing otherwise. This approach would solve a number of problems, including budget problems and the question: Who owns the technology? If the free enterprise system means anything, it means that an economic incentive route is probably the most effective way to get the commercialization job done.

Discussion

It was the consensus of the speakers and participants that the demonstration process is becoming more prevalent, and that it is attractive in moving new technologies more quickly into the marketplace. But a number of significant problems have yet to be solved.

Demonstrations are usually performed under the hidden assumption that they will succeed. There are two ways to stimulate success: 1) have the federal government pay front-end costs, and 2) build in incentives such as tax breaks. If the second route is taken, however, it is very difficult to stop a demonstration, and one element in R&D

budgets should be some provision of a "self-destruct mechanism" for demonstrations that either don't work or prove to be not particularly useful.

There is a widespread belief that if the federal government becomes actively involved in any project, that project will be delayed rather than facilitated. If federal support follows through to demonstration, success can be built in by using federal funds to pick up what has been demonstrated. This leads to the very real issue of setting an acceptable failure rate for demonstration projects. All basic research projects are not expected to be successful. However, demonstrations for commercialization are expected to have a better chance for success. One should not demand 100 percent success though, for even if a particular demonstration program fails, the information gained may save federal funds by showing that that particular process is not cost-effective and ought not to be diffused.

Policy-makers frequently hope to have _a priori_ knowledge of the results of R&D programs. But programs can fail at other stages as well as at the technological development stage. There can be failures that result from making premature demands on a new technology only to discover, at some later point, that the original suppositions were wrong. This is a problem at the state level, as well as at the national level: Legislators are unwilling to work with the uncertainties that scientists and engineers accept as normal.

Too much can be asked of demonstration programs designed to get a technology commercialized in the private sector. If too much _new_ information is required (e.g., about basic technological performance), the project is not likely to provide a successful example of the use of the technology.

The desirability of striking a balance between demand pull and technology push was discussed at some length. It was noted that supply demand balance models are very misleading. Although 75 percent of demonstrations happen as a result of demand pull and 25 percent occur because of technology push, it must be realized that a great deal depends on the nature of the organization involved in the program. In many demonstrations it is insufficient to demonstrate only technical and economic feasibility. The demonstration must also contribute to political accommodation.

Most demonstrations are aimed at specific users. Frequently there is a failure to recognize that some social accommodations will be necessary if the demonstration is to have its maximum possible impact. In demonstrations for regulatory activities, there should be a consciousness and perhaps even an analysis of the consequences and side effects of adopting particular social policies.

Social demonstrations have the advantage that interested parties are involved in the design of the demonstration, and political negotiations occur throughout the process. The same types of political negotiations do not normally occur in closed engineering processes, at least in the early planning stages. However, if a technological demonstration project were planned as part of an ultimate political accommodation, and if the people involved were included in the early stages, it might be possible to develop a program to serve both the commercialization function and the problem of political feasibility.

But there may be a danger in thinking that ERDA's intent for commercialization programs is totally different by nature from the intent of other R&D programs. It was suggested that the difference is not so much one of nature as one of degree. This is a concept of considerable importance since a difference of degree often implies a difference in kind. Ultimately, successful demonstrations must do more than show reliability. Comparative cost advantage is of paramount importance -- especially in persuading an industry to adopt a demonstration. The industry must be shown that the technology being demonstrated is potentially more profitable than any other course that could be taken.

Workshop 3

R&D BUDGETS AND TECHNOLOGICAL ADVANCE

Participants

Presiding: **John M. Logsdon**, *Director, Graduate Program in Science, Technology, and Public Policy, The George Washington University*

Rapporteur: **John H. Young**, *APS Congressional Science Fellow with the Senate Committee on Commerce*

Prepared Presentations: **Lowell W. Steele**, *Consultant, Planning for Corporate Research and Development, General Electric Company;* **William M. Capron**, *Associate Dean, JFK School of Government, Harvard University*

Invited Comments: **Klaus P. Heiss**, *President, Econ, Incorporated;* **Robert L. Stern**, *Director, Business/Government Relations Research, The Conference Board;* **Michael L. Telson**, *Energy Analyst, House Committee on the Budget*

R&D Budgets and Technological Advance

Introduction

John M. Logsdon opened the session by noting that the relation between federal R&D expenditures and technological advance in the private sector would be explored in the workshop.

Prepared Presentations

Lowell Steele pointed out that the relationships between R&D budgets and technological advance are so complex that he would concentrate only on industrial perceptions of those relationships. Most people who have thought at all about these issues seem to agree that R&D is socially valuable. However, that consensus does not get us very far. It has to be recognized at the outset that R&D is not an end in itself. Rather, an inquiry into the uses of R&D must be the key to approaching the workshop topic. Researchers who are always on the supply end of the R&D utilization process have a perspective that is quite different from that of potential users. Since industry must focus on the end use of R&D, it follows that, from the industry perspective, market pull must dominate over technological push.

Steele emphasized that the process linking technical capability to eventual end use is exceedingly complex. In response to the question, "What are the barriers to applying a new technology?" R&D managers generally respond "Costs," which involve not only the direct costs associated with the specific application of the new technology, but also the indirect and ancillary costs associated with fitting the new technology into the system. Many things are technically possible which are not economically feasible. We frequently underestimate the amount of knowledge required to make something as simple as a refrigerator on/off switch work reliably. In addition, we often forget that technology has no autonomous existence. Rather, it survives and prospers within a network of supporting structures. We tend to associate innovation with something new. However, most innovation is pushing aside something older to provide a better way of doing things, and consequently innovation is aiming at a moving target because the older way of doing things can improve during the process of development.

Steele gave two examples to illustrate the difficulties of technological innovation: the introduction of frozen foods and the greater use of plastics.

The initial discovery that food could be preserved through fast freezing was made quite accidentally by Clarence Birdseye in 1912, while he was ice fishing. Having made that discover, however, he still didn't know the nutrient value of frozen food, its aesthetic value, acceptability to the consumer, or how to freeze, store, transport and market frozen foods. By 1930, Birdseye had pretty well solved the questions of how to freeze rapidly, of nutrient value, and of consumer acceptability. But a whole new industry had to be created in order for frozen foods to come into widespread use. New arrangements had to be made with the farmers who provided the food; canners had to become freezers; ways of transporting frozen foods had to be developed; grocers had to have storage space in which to place the freezers for the food; and, finally, the consumers had to have ways to store the food. An enormous infrastructure had to be created in order for this technical advance to be adopted.

These changes would not have occurred without the advent of supermarkets, the redesign of refrigerators, and larger kitchens to accommodate the larger refrigerators. The social changes that create both opportunities for and revenue from this technological advance still continue as more women enter the job market and create a greater demand for convenience foods.

The second example Steele gave to demonstrate both the difficulties of technological innovation and the effects of successful innovation on the entire social structure was the growth of the widespread use of plastics. When Dupont first developed nylon in the late 1930's, its use was limited to such items as stockings. However, the mechanical, thermal, and abrasive properties of plastics soon suggested their potential for replacing brass, zinc, and aluminum in numerous applications. However, the plastic molding industry only manufactured very small parts, suitable for ram-molding techniques, and available die designs were extremely crude. There was no incentive for zinc die casters to work with plastic molders to develop better methods, and, since nylon requires molding temperatures 150° F. hotter than metal, and always picked up water when dried, there were severe technical problems that had to be overcome.

Ultimately, the development of screw injection molding allowed greater pressures to be developed which, in turn, allowed larger parts to be molded from plastic. This created a new market for injection molding which, in turn, stimulated machine tool manufacturers. Thus, manufacturers had to be convinced to adopt major, often unproven, ways of doing things in order for greater use of plastics to become widespread.

These two examples illustrate the need to sort through the industrial maze to find the proper niches before the utilization problem can be solved. In order to provide incentives to industry to invest its resources in carrying through to utilization, there have to be both economic awards and a societal value system that provides status to innovators. The federal government has shown little understanding of these downstream problems. Rather, its role has been limited to creating technical capabilities which cannot lead to innovation without an understanding of the broader economic and social contexts of technology.

William Capron began by suggesting that economics had more than justified the "dismal science" label that Mill had applied to it. Indeed, the contributions made by economists to an understanding of the relationships between technological advance and economic growth has been a very limited one. An article by Edwin Mansfield in the February 4, 1972 issue of Science* surveyed what was known about this relationship, and not much has been added since that time.

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*Mansfield, Edwin. "Contribution of R&D to Economic Growth in the United States," Science, 175, No. 4021 (February 4, 1972), 477-86.

Capron said that three factors have accounted for much of the economic growth in the United States: 1) an abundance of natural resources, 2) the development of high technology, and 3) strong incentives to develop and, more importantly, to apply this technology. He felt that agriculture has been the area where high technology has been most important. The United States has also done quite well, internationally, in the areas of communications, aerospace, and computers. However, there is always an argument whenever the federal government is asked to support R&D outside traditional areas such as agriculture, as the often acrimonious debates about nuclear power and the SST indicate. Capron thought it would be safe for economists to assert that a competitive economy will "underinvest" in R&D because much of the benefits cannot be captured by the innovator.

Concerning federal funding of R&D, there are two main points to consider: 1) very different considerations apply for "public goods," such as national defense, as opposed to innovations that will ultimately appear in the marketplace, and 2) the toughest problem is operational; that is, the transfer of technology from government to a commercial venture. The government does the poorest job in the latter area. And there is the difficult question of who will reap the profits. That is, should federal R&D investment policies be focused on the domestic economy in a narrow sense? Or should they be conceptualized in the broader context of the public interest?

Capron said that thus far economists have provided little guidance to the Office of Management and Budget (OMB) in formulating R&D budgets. In particular, science indicators have yet to provide much help in this regard.

Margaret Mead interjected that in moving from primitive to productive societies, entrepreneurs set up a calendar to mesh their activities, and that in socializing their resources they reduce both the efficiency for development of these resources and their social tensions. She felt that we should look at the whole system, social and political as well as technological, and that it would be useful to make comparisons with societies such as the USSR in this regard.

Invited Comments

Klaus Heiss directed his comments to the role of federal and free enterprise in technological innovation. He felt it was important that entrepreneurs be allowed to reap the benefits of their invention as well as taking the risks of going out of business. Innovation is the source of profit and advance of economic systems, and lack of innovation leads to stagnation, loss and bankruptcy of non-innovative systems. He cited the economist Schumpeter as providing deep insights into the driving force for innovation in economic systems.

The problem with the federal process, Heiss felt, was that it did not provide adequate economic accountability -- checks and balances of the type the market provides in private enterprise. The present OMB "assessment" of R&D is hardly more than a year-by-year watch of budgetary numbers with little, if any, analytic effort or broader understanding of the role of federal R&D in the economic system. Most of these budgetary numbers result from historical accident. Nobody really has addressed the question of whether the United States is doing enough, too much, or too little R&D at the federal level in an analytically convincing way. OMB and the federal government do not go bankrupt; hence there are no built-in risks to making wrong R&D decisions or none at all.

In the context of OMB R&D planning, Heiss posed the question: Should R&D be a "controllable" budget item from year to year, subject to the whims and accidents of fiscal and monetary policy, or should R&D be seen in a longer-term context, grouped under the category of "uncontrollable" in the sense that, once a four-year or eight-year outlook assessment on the general level of federal R&D financing has been made, these budgets would not be subject to arbitrary annual change? He felt that there is a need to engage in long-term R&D economic assessment at the federal level so that the best use can be made of the scientific and technological potential of the United States both in government and private industry. Heiss thought that a concrete, specific assessment of these programs can be made in nearly every instance. This does not mean central planning of research and development, but rather would involve a continued assessment of the level of financing of federal and industrial R&D, to best use and develop the scientific and technological manpower of the United States.

As the current lack of analytic economic assessment of R&D is continued, Heiss felt pessimistic about the future of Western economies. He cited evidence of a slow whittling away of the technological and capital bases taking place in these nations, and termed it "the British disease." There is nothing inherent or deterministic about this in free market economies. However, what is required today is innovative and new thinking at the federal level and ways to stimulate and incentivize the market system through the federal government to continue the history of technological innovation and change of the United States.

Heiss concluded by noting that there is a strong need for the AAAS to provide leadership and initiative in this area, since nobody else will do this thinking for the scientific and technical community.

Robert Stern began by saying that, although economics may be a dismal science, economists have performed an unrecognized service for technologists in trying to articulate the relationship between technological activity and economic growth. He pointed out that during the early days of the Kennedy Administration people like Bill Capron, Dick Nelson, and others in the Bureau of the Budget and the Council of Economic Advisors were concerned about articulating the relationship between technological activity and economic growth, and they were able to raise these discussions to a level comparable to discussions about the impact of fiscal policy and monetary policy. He thought it was unfortunate that the role of technology in the economic affairs of the private sector is still relegated to a lower position than in government circles, but thought that might change because of what is happening to the federal R&D budget; that is, the growth part is in civilian oriented activities.

Stern thought that was encouraging because we are seeing the growth of at least the availability of money in those areas, but he pointed out that tremendous energy and persistence is needed to get technological development into the private sector and make it of commercial significance. But it is difficult to sustain a technological effort that is federally budgeted over the necessary time span because the federal budget cycle is annual and administrations change frequently. This should be a matter of great concern. Stern thought the AAAS could serve as a meeting ground for the private sector and the public sector on the proper administration of the parts of the R&D budget that do not relate to activities of individual companies, but rather are aimed toward the "public goods" that Capron had mentioned.

In most of these areas there is a need to interact with the whole infrastructure. But a certain dynamic tension must be maintained and our society is getting into the area of public support of R&D where the trade-offs will be difficult to make. AAAS could provide a forum for discussion of these matters. Stern concluded by remarking that wealth awaits us if we succeed in this endeavor, poverty if we don't.

Michael Telson noted that the relationship between government R&D activities and private sector technological advance was not a simple one to understand and that our shortcomings in this respect had become increasingly apparent as we have sought to accelerate the process of technological transfer in the energy area, as would seem to be desirable. Aside from the substantive problems in understanding the relationship, Telson felt that at present there is a particularly high level of distrust between government and industry that makes it very difficult to address rationally questions relating to the proper relationship between the private and public sectors in this area. As an example, he cited the high degree of controversy surrounding the various synthetic fuel proposals in Congress. He felt that the public had initially regarded these as "rip-offs." As a result, the arguments had become so heated and polarized that it was nearly impossible to get a reasoned discussion over the proper roles that government and industry should play over time in developing an economically attractive synthetic fuels production capability for the nation.

Telson felt that the AAAS and other such societies, because of their reasonable claim to objectivity, and because of their long-term interests in the more informed resolution of these problems, could and should play an important role in helping to refine and define the proper bounds and techniques for fruitful government/industry interaction. Too often decisions are made in this area which are based solely on the momentary political power of either side without active, or even passive, involvement of other interests. As a result, no one knows how to carry out successful long-range planning very well. Yet it is clear that many government agencies, particularly the Energy Research and Development Administration (ERDA), need considerable assistance in this area.

Telson disagreed with Capron's thesis that competitive industries would necessarily underinvest in R&D; he especially disagreed with the implication that noncompetitive regulated industries would invest more. He cited the example of regulated electric utilities which until recently had a poor record of performance in R&D, and how this differed from the Bell Labs example implicit in Capron's thesis. Telson suggested that the differences in the behavior of these two types of companies had complex sociological roots and could better be explained in terms of historical accident rather than solely in terms of economic determinism.

Capron replied that it was important to distinguish between an organization like Bell Labs and the public utilities which are very different. He felt the utilities have a very different relation with the state power commissions than AT&T does with the Federal Communications Commission. Although productivity of the utilities has increased remarkably, they have relied on manufacturers of electrical equipment such as General Electric and Westinghouse. Capron felt this was basically a problem of management leadership. Capron also thinks the annual budget cycle is a terrible handicap for the planning of federal R&D. He would prefer multi-year authorization.

Discussion

The discussion covered a range of topics. The first question concerned the difficulty government has in transferring technology to the private sector. The government patent policy is clearly reluctant to let a private entrepreneur gain monopoly profits on an invention. Steele replied that, whereas many consider such transfers to be offering private industry a golden goose, he felt it is more a ticket to take a risk, as illustrated by his examples of technological innovation.

It was asked whether industry management does not take a shorter-term perspective than necessary for technological innovation. Steele replied that this is a problem, and that it is compounded by inflation. However, in the past decade, companies have also learned a great deal about their real limitations in this regard. He felt that the General Electric management has a true sense of stewardship. But he noted that management and technology skills are often very specific to a particular company, and therefore not readily transferable outside that company.

During a discussion about the difficulties associated with the large amounts of money required for energy R&D, Stern said that in his opinion the creation of ERDA was a very big blunder because it removed the need for industry itself to come to terms with the energy crisis and the problem of developing energy technology. A great deal of uncertainty has been created by the government assuming responsibility in this area.

A participant from OMB noted that the criteria for allocating federal R&D dollars are very different from those in the private sector. He pointed out that the government's response to OPEC was to increase energy R&D funding by $2 billion. This inflates the cost of doing R&D in the energy area, and eats up the private sector response.

In summarizing, John Young noted the lack of connection in the discussion between the federal R&D budget on the one hand and technological advance on the other. Both topics were discussed, but the coupling between them was weak. In view of the difficulties noted by the conferees in commercializing technologies developed with federal support, he felt the absence of a discussion of alternatives, such as tax incentives, was quite noticeable. The rationale of underinvestment would presumably apply to such alternatives as well as to the federal support of R&D. Another alternative is to try to improve the environment for technological innovation in the private sector through the modification of government policies, which is the approach of the Experimental Technology Incentives Program.

Young also noted that a recurrent theme had been the question of incentives. The use of private gain to achieve public welfare appears to be becoming increasingly suspect in our society.

Note Added in Proof: The Subcommittee on Domestic and International Scientific Planning and Analysis of the House Committee on Science and Technology heard testimony on Federal Research and Development Expenditures and the National Economy on April 25 - May 5, 1976. A report on these hearings, including a critique and analysis by the Subcommittee on Federal R&D Expenditures of the AAAS Committee on Science and Public Policy, is available from the House Committee.

See also: Walsh, John A. "R&D and Economic Growth: Renewed Interest in the Federal Role," Science, 193, No. 4258 (September 17, 1976), 1101-03.

Workshop 4

R&D BUDGETS AND LONG-RANGE PLANNING

Participants

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R&D Budgets and Long-Range Planning

Introduction

Brewster Denny opened the workshop by listing four broad issues, touched on at the opening plenary session, that could provide the context for exploring R&D budgets and long-range planning:

First, societal and institutional issues and objectives underlie federal R&D budget processes. Changes that affect the U.S. economy and the relationship of the economy to U.S. society are certainly implicit in questions regarding long-range planning for R&D. Research and development play a central role in several critical areas where both the public and private sectors are deeply involved.

Second, long-range planning is bound to have important institutional impacts. Over its history, U.S. policy has operated on the basis of two, sharply differing perceptions of universities: 1) as a set of institutions that could be contracted with to produce products useful to the federal government; and 2) as institutions that are fundamental resources to be supported and encouraged because they are essential to the operation of society as a whole, and to the research and development enterprise in particular. The long-range policies the federal government adopts regarding university R&D depend critically on which of these perceptions is followed. State and local governments represent another set of institutions important in long-range federal R&D planning, but their capacity to deal with technological problems is generally minimal at best.

Third, one's approach to the question of long-range planning for R&D will differ considerably depending on whether one assumes a "budget ethic," implying that dissatisfaction is to be allocated among competing claimants, or whether one assumes what might be called a public interest ethic.

Finally, it is obvious that one's view of what planning is defines one's approach to planning. The Shapley report does not deal with planning as a blueprint or a computer program. Rather, it deals with planning in the spirit of Harlan Cleveland's definition: an improvisation on a general sense of direction.

Prepared Presentations

Ellis Mottur noted that, since Congress is not a monolith, there obviously can be no single congressional perspective on any issue of substance, including long-range planning for R&D. He admitted that his own approach to long-range planning for R&D rested on a number of assumptions.

First, the country is in great need of more long-range planning. Indeed, the prevailing bias against government planning is itself a serious problem.

Second, the nation has been underinvesting in R&D, and we have yet to find adequate mechanisms for making civilian R&D as responsive to social and human needs as it should be.

Third, planning for basic and non-directed research is quite different from planning for applied or directed research, and also from planning for development and demonstration. In approaching the problem of long-range planning for basic research, it is first necessary to try to determine the parameters that define a healthy scientific enterprise and then plan long-range R&D investments to keep that enterprise healthy.

In Mottur's opinion there has not been much significant long-range planning for R&D in the country. The National Aeronautics and Space Administration (NASA) has probably been more successful in this respect than other government agencies, since it has a clearly defined set of missions and has dealt in areas where the basic research knowledge is fairly well known.

Mottur agreed with Shapley that long-range planning for R&D may be a mixed blessing. Formalized planning systems tend to force premature decisions on future courses of action and thereby reduce the options available for exploration when opportunities arise. In addition, it is not effective management to incorporate in long-range plans negative events such as anticipated program cutbacks and terminations.

The major problem that must be faced is that no one really seems to know how to carry out effective long-range planning for R&D. Budget planning processes depend on the interrelationships among four elements:

1) Certain explicit goals that one hopes to accomplish;

2) External conditions that may undergo uncertain changes in the future;

3) Sets of institutions and mechanisms for implementing program plans; and

4) Various resources which those institutions can draw on to meet explicit goals of the program plan.

Unless one understands the relationships among these four elements from the perspective of R&D, long-range planning cannot be carried out effectively. It follows that the most important present objective should be to develop the necessary understanding of the R&D process so that we will be in a position to do more long-range planning.

Mottur then turned to two recent developments that have potential for developing the requisite understanding, and which also involve institutional developments that will help shape the way planning is done. The first of these is the establishment of the White House Office of Science and Technology Policy (OSTP); the second, a program recently initiated by the Office of Technology Assessment (OTA).

The legislation establishing OSTP has two major provisions that are germane to long-range planning for R&D. Section 303 establishes the President's Committee on Science and Technology and gives it the mandate to conduct a major, two-year survey on federal science, engineering and technology with an interim report due after one year.

That survey will consider federal programs in terms of organizational reform; improvements in existing systems for handling scientific and technical information; improved methods for technology innovation, transfer and use; mechanisms to stimulate more effective federal, state and local relationships; and means to establish a broader base for the support of basic research and to strengthen the nation's academic institutions. It is hoped that the survey will provide a useful framework for future long-range planning for R&D.

Section 206 of the law contains two important provisions. First, it requires that the White House science advisor look ahead five years, and try to develop a set of viable options for federal R&D programs based on that five-year outlook. Second, it gives the science advisor the function of introducing those options into the budget development process in the Executive Branch. The five-year outlooks are to be revised annually to take into account new circumstances and opportunities as well as changing national goals.

The OTA recently launched a major R&D policies and priorities program in an attempt to develop that better base for understanding R&D which is a prerequisite to effective long-range planning. It is intended that the program will develop criteria that the congressional Budget Committees can use to evaluate R&D budgets, as well as new legislative alternatives dealing with matters such as federal incentives for R&D to private industry.

Mottur concluded by reiterating his key point: We do not really know how to do long-range planning for R&D, but some recent developments have the potential for beginning to give us the understanding that is required.

Hugh Loweth, the second invited speaker, focused his attention on several problems inherent in long-range planning for R&D.

Referring to Mottur's assertion that NASA has done some effective long-range planning, Loweth noted that such planning is relatively easy in a closed system in which the government is both sponsor and user of the research. Planning is much more difficult when the federal government is doing research in order to accelerate what the private sector should or would do.

The NASA example raises the issue of whether what can be done really needs to be done and therefore is affordable from a long-range perspective. At the Office of Management and Budget (OMB) it is always assumed that a technology will work. The next question is, what will we do with it? Probably both technology push and demand pull are necessary, but an exceedingly costly push must be questioned.

Loweth also took exception to Denny's assertion that planning should be regarded as an "improvisation on a general sense of direction" since, in his experience, that approach can often be reduced to a rationale for throwing money at a problem.

The energy area provides a set of examples that illustrate the pitfalls of long-range planning where the federal government is involved in someone else's R&D. The questions of what energy-independence really means and when it is meaningful do not have simple answers.

The temptation for Congress to put money into currently fashionable research also makes long-range planning exceedingly difficult. The President's FY 1977 budget provides about $160 million for solar energy research, a 40 percent increase over FY 1976. Yet Congress shows signs of wanting to increase that amount to $300 million. In Loweth's view, many congressmen who refuse to bite the bullet on tough questions such as natural gas deregulation have become champions of solar energy, possibly to salve their consciences.

There is already a law that requires the Executive Branch to submit five-year plans for every piece of legislation enacted. OMB tries to get the various agencies to take these plans more seriously so it can get a handle on out-year costs. It is easy for Congress to invent a program with a low entry cost and not worry about out-year implications. A $1 million program in 1976 can easily build to $10 million by 1977 and $100 million by 1978. The new congressional budget process suggests that Congress wants to exert a measure of current-year discipline upon itself. It remains to be seen whether it will also begin to worry about out-year implications.

Loweth took exception to the notion that basic research cannot be planned by noting a distinction between planning of and planning for. The continuing expansion of big science dictates the need to do more planning for basic research. For example, we cannot randomly continue to build more large-scale accelerators. If we invest in expensive national centers for a particular field, we may not be able to continue to support many academic institutions in that field. The fact that $400 million may be budgeted for a space telescope should say something to astronomers about their chances of getting expensive ground facilities. The scientific community will not only have to accept facts such as these, but will also have to make some tough decisions itself. The National Science Foundation (NSF) has become a big agency without quite realizing what has occurred. It will have to accept accountability for the stewardship of basic research funds, and the scientific community will have to help the NSF show that we are not just "throwing money" at basic research.

Invited Comments

Russell Drew suggested that a more explicit recognition of the policies used in the budget process would be a short-term improvement which, in his view, would lead to many of the alleged benefits of long-range planning. Shapley's report removed much of the mystery that has shrouded the budgeting process, and that in itself should help people on the receiving end understand the way decisions are made in that process. However, claimants to a share of the federal R&D budget face the problem that OMB performs a very skillful sleight of hand by never making the current decision-making policies completely explicit. Thus long-range planning for R&D is hampered by the fact that various federal agencies are forced to participate in an adversary process without really knowing the ground rules. The new OSTP could play a constructive role in the R&D budgeting process by setting explicit policies and trying to insure some commitment to those policies.

In response to Drew's comment, Loweth admitted that, historically, budget documents lacked explanations of why particular decisions were made or what those decisions foreshadowed. However, this year the OMB and the Domestic Council produced a document called Seventy Issues which attempts to explain not only what has been done, but why it was done, and what the future implications are for seventy key issues, including energy and R&D in general.

Loweth also suggested that since there is so much give-and-take in the budget process a certain amount of mystery is inevitable. Setting explicit policies with respect to specific areas is particularly difficult. The problem OMB faces is one of perspective, taking into account the need for breadth of views.

Edward L. Helminski noted that since the late 1960's federal policy supposedly has been to strengthen the capacity of state and local governments. However, no funds have been forthcoming to build state capacities in the area of science and technology, thus making it appear that OMB does not take that policy seriously. In fact, the federal government has actually reneged on the funds needed to strengthen the states' role.

Nevertheless, according to NSF, state-funded R&D has reached the $230 million level. Most of those funds support research and development directed toward state problems. From Helminski's perspective, the most pressing issue with regard to long-range planning for R&D is to create mechanisms for getting the states and cities involved in setting federal priorities.

A few states are considerably ahead of the federal government in developing R&D programs in several rather specific areas. Maryland has formulated a long-range planning program for future power-plant construction. California was the first state to regulate auto emissions and has recently adopted stringent legislation that could impact further nuclear power plant construction in that state. Oregon has banned the use of aerosol flurocarbons and has taken the lead in long-range environmental policy planning. These examples show that several states are concerned about appropriate uses of science and technology. Yet, in general, they lack the capacity to set national priorities. Very few people at the federal level seem concerned about developing the capacities of states and cities to get involved in R&D planning processes. In those states where the capacity does exist, there does not seem to be an interest in taking advantage of it at the federal level.

Glenn Schweitzer prefaced his remarks with the reminder that large segments of U.S. society are turned off by the ever growing size and reach of government.

Congressional unhappiness manifests itself in actions affecting program authorizations and regulatory procedures that are resulting in a major transformation of the way the federal government does business.

The American Society for Public Administration (ASPA), acting through its Committee on Science and Technology in Government, was involved in the development of the legislation to establish OSTP. Planning in the White House -- and throughout the government -- has been poor. For several years OMB and/or the Federal Council on Science and Technology have had the opportunity to produce genuine planning documents that could be of help in the federal government and elsewhere. But the documents that have emerged are shallow budget justifications lacking any planning utility. ASPA has voiced its opposition to the fact that the two White House science advisory committees appointed in 1975 were composed exclusively of scientists, engineers, and economists, with no representation from other broad sectors of society. ASPA has documented the interactions between technological innovation and regulation in the economic and health and safety areas in order to demonstrate that budgets are but one of several federal activities important in planning for science and technology.

Schweitzer made three points regarding White House-directed planning for science and technology: First, no one should be overly optimistic about what governmental planning can or should accomplish, particularly when the private sector is involved. Second, it is essential to remember that planning is now carried out by a number of White House offices and advisory groups as well as by the agencies themselves. It would be well to examine how some of the White House groups function, and particularly whether their planning documents have any impact. Third, we should know what we are planning for: Are we planning for science and technology, or are we attempting to illuminate the scientific and technological components of major societal decisions? He feels that most of our planning is too heavily oriented toward technological hardware.

Schweitzer made five suggestions regarding the future activities of OSTP. First, it should not try to cover every interesting issue, but instead should select a few issues it can handle well. Second, it should reinforce and not usurp the planning prerogatives of the agencies.

Third, it should emphasize that budgets are only one of several tools available to the federal government. Others include procurement policy, taxes, regulations, and import-export policy. Fourth, if OSTP plans to develop mechanisms for public participation in decision-making processes, it must be genuinely interested in utilizing such inputs. Finally, OSTP may well have a unique opportunity to demonstrate to the rest of the government a new style of public accountability, provided it receives the necessary support from the broad community of scientists and engineers.

Schweitzer concluded his remarks with a comment on the effects of international science and technology agreements on budgeting for R&D. These agreements are almost always politically inspired. But there is always the problem of who pays the bill. Perhaps the only realistic way to accommodate this dimension in the budget process is to establish a slush fund that can be utilized with a short lead time.

Joseph H. Engel focused on what universities can or should contribute to the long-range planning process for R&D budgets, and what they can or should get out of that process.

Universities already contribute directly to the R&D budget process by providing consultants to federal agencies engaged in the various activities that are parts of the long-range R&D planning process. Universities contribute indirectly by research which examines the scientific enterprise and attempts to define or clarify various research options that should be explored.

Universities do not participate as well as they might in long-range planning for two principal reasons.

First, they are not the sole repository of basic research capacity. Many private industries, nonprofit institutions, and federal government agencies have also built up their basic research capabilities. Indeed, in several important fields non-university research laboratories may be preempting universities.

Second, we lack adequate ways to measure the capabilities and contributions of any particular R&D program.

Nor have we really defined exactly what those programs should accomplish. One method for arriving at some objective measures would be to examine some of the contributions made to specific fields by science and technology over long periods of time.

Helminski supplemented his previous remarks in light of Engel's comments. It has been Helminski's experience that many people overlook the fact that the basic operating budgets for state universities are appropriated by state legislatures. The degree to which these universities participate in federal planning processes depends critically on the basic support they receive from their states, and that is an important factor in assessing the total R&D contributions made by state governments. Yet few universities realize their potential for becoming assets to their states in R&D matters. Indeed, most follow the federal dollar and are more than happy to alter curricula to suit the current fashion defined by available federal grants. Universities do not tend to be concerned with identifying problems and priorities in R&D at the state level that they might be capable of handling, nor do they seek state or federal funds that would permit them to develop those capabilities. Both universities and states must recognize that state universities can be important assets to government, especially state government, but that some tough decisions will have to be made for them to realize their full potential.

Before opening the session to general discussion the chair called on Alan Smith, Counsellor for Science and Technology at the British Embassy, for some impromptu remarks.

Smith confessed that he did not find the words science and technology particularly useful in discussing international trade. Governments can and have distorted that aspect of the international market both by unilateral and by collective actions. In the 1960's Europe lost almost all its aerospace engineers to the great upsurge of aerospace projects in the U.S. There is some fear that the same thing may happen in energy-related fields, if the U.S. continues to throw money at those problems.

If one insists on referring to science and technology, then the international dimensions of the two related activities should be distinguished. There is nothing inevitable about the internationalism of science, particularly basic science. Since the primary objective of basic science is to develop new, often unanticipated kinds of knowledge, scientists regard communication between people working in the same or in related fields as essential to the enterprise. Scientists have decided to communicate internationally, and thus to make science an international activity. Occasionally, national governments intervene by restricting travel of their own nationals or by supporting or failing to support a particular field of research. These actions certainly effect the development of that field in its national setting. However, they do not seem to alter international progress.

Technology, in contrast, has economic, social and political implications that impress its own imperatives on national governments. Nearly all of them are beyond the control of any individual government. It is difficult, as well as potentially hazardous, for a national government to slow the pace of technological change. Nations that have been successful in doing so have lost ground in international commerce and competition.

Discussion

Much of the discussion concerned the necessity of conceptualizing long-range planning for R&D within a broad social context. The starting point should be to learn how to intervene in decision-making processes in order to produce more socially satisfactory outcomes. Many people who have thought about the problem now advocate management by results rather than management by objectives. For example, environmental planning ought to strive for an environment which people will use in a better way rather than simply for a clean environment. If long-range planning is to be carried out in a broad social context, it may be important to recognize that research and development are not equivalent to science and technology. Such an assumption could lead to an insensitivity about social wants and needs. In the same vein, it cannot be assumed that planning for research and development should proceed in parallel, for the goals, participants, decision processes, and consequences of mistakes are quite different.

There was some discussion of long-range manpower planning, particularly in higher education. Bright young people will not prepare themselves for careers in science and technology if they believe that there will be few satisfying opportunities available. Thus the present and near-term health of institutions involved in research and development can have a considerable long-range impact on the entire scientific enterprise and its relationships with the broader society. There is also considerable disparity between the trained manpower that is available and that which is needed. For example, the need for more basic research in agriculture has been pointed out repeatedly. But the academic reward system is such that even universities in largely agricultural states do not exhibit much interest in agriculture. The research and teaching priorities of the universities could be changed somewhat if federal funding agencies changed their priorities, and if state and local governments were encouraged to develop their capacities to make better use of the potential intellectual resources available at their universities. While many seemed to accept the desirability of long-range planning in R&D, participants were also reminded by representatives from industry that the business community is wary of long-range R&D planning, as it might be the first step in major federal intervention in national economic planning.

Several conditions were suggested as prerequisites for successful long-range planning: 1) recognition of the function that planning is expected to assume, 2) reasonable insulation of the planners from short-term "fire fighting" tasks, and 3) the provision of adequate resources. If the scientific community is to do more than pay lip service to public accountability, it must become more involved in long-range R&D decision-making processes. But, as Mottur pointed out, we still lack the basic understanding required for significant long-range planning. Research is required to gain that understanding, and research requires money. The federal government could demonstrate its good intentions by investing a small fraction of R&D expenditures to help the scientific community acquire that understanding and develop the capacity to involve itself in long-range planning processes.

FINAL SESSION

William D. Carey, presiding

Executive Officer, AAAS

One objective of this colloquium has been to have everyone understand that budget-making is not the monopoly of the President as we in the old Bureau of the Budget believed firmly to be the case. We always thought it was Congress' duty simply to fall in line and do what the President told it to do. However, things seem to have gotten out of hand a little bit since those days. I think it is fair to say that the budget now is seen as a key instrument in the policy process both in the Congress and the Executive Branch.

The institutional disarray which hampered the Congress in past years in dealing with the budget certainly is now in the process of being corrected. Whether Congress can digest the whole meal that it has prepared for itself is a very lively question in Washington. My own view is that we can't afford to have the process fail and, as so often turns out to be the case, its success depends on a handful of people in key positions in both the House and the Senate who are backed up by their professional staffs.

One of those people is with us today. Since the new Budget Reform Act went into effect two years ago he has been on a hot seat. Sam Rayburn used to tell young congressmen the way to get along is to go along, but it is tough to take that advice when you are faced with rationing money and turning down your colleagues in the House. Back in the old Budget Bureau days we used to have a saying that the budget is a process designed to insure the uniform distribution of dissatisfactions. I don't think that's changed and I think Brock Adams knows how true it is.

Congressman Adams brings to Congress a background in economics and law. He took a summa cum laude in economics from the University of Washington, and his law degree from Harvard. President John Kennedy appointed him District Attorney for Western Washington in 1961. He was elected to Congress in 1964 and is now rounding out his sixth term.

As Chairman of the House Budget Committee I assume that Brock Adams doesn't have very much time to mend his fences back in the 7th District of Washington, and so we can hope in a nonpartisan way that his constituents will make due allowances and will vote early if not often for budget reform.

Mr. Adams, you know with $25 billion a year in the President's budget for scientific research and development, the government's policy laws and budget preferences go very far to define the direction and quality of this country's research and development. This colloquium is really concerned with the capacity of the national policy machinery to set goals and priorities and to allocate resources more accurately and well. The problem of coping is one in which you are deeply involved. The congressional budget process is a very important new element in upgrading that policy machinery and we are delighted and complimented that you'd come down here on a busy day to share your perspectives with us.

Brock Adams
Chairman, House Committee
on the Budget

Federal R&D Spending: The Potential Impact of The Congressional Budget Process

It is a pleasure to join you this afternoon at the first AAAS colloquium on funding for research and development. I applaud your efforts to begin a new, annual R&D budget analysis project. It is a project which I believe will both reward and exhaust you with endless budget experiences. If, as Cervantes wrote, "experience is the mother of sciences," then I am but a budget apprentice. However, the past year and a half as House Budget Committee Chairman has taught me one scientific lesson -- if one doesn't carefully handle all the ingredients, the whole concoction can blow up. I hope, in my remarks, to give you some idea of the chemistry of this new budget process.

At this stage in the colloquium, I want to focus on four topics: first, the new congressional budget process; next, I will discuss how R&D is and is not treated in this new process; third, I'll talk about what I think you can do to effect R&D spending; and fourth, I will raise two enduring budget problems which I believe will have continuing impact on all R&D budgeting. These are the problems of:

1. Budget Control -- How do we get a better grasp each year on the size and focus of budget expenditures?
2. Impoundment -- The withholding by the Executive Branch of congressionally approved spending.

The Congressional Budget Process

This year, 1976, is critical for the congressional budget process -- a process designed to accomplish two major purposes:

First, Congress is responsible for developing a coherent fiscal policy, in advance of each fiscal year, by deciding how much money in aggregate the federal government should spend, how much it should take in, and what the surplus or deficit should be in light of projected economic conditions.

Second, Congress is responsible for deciding in overall terms what the major national budget priorities should be. These decisions on priorities are reflected in the relative shares of the federal budget assigned to each of sixteen broad functional categories.

This dual decision-making procedure for Fiscal Year 1977 began in November, 1975, when the President submitted the so-called "Current Services Budget." This "Current Services Budget" shows the total spending that would be needed to operate all existing federal programs in the next fiscal year at the same level as this year. The current services budget was followed by the submission in January of the President's own budget -- a budget which reflected dangerous reductions for many programs. For all research and development, President Ford's budget requests, adjusted for inflation, would have barely maintained the status quo.

The House Budget Committee immediately began extensive public hearings on both the President's budget and alternative proposals. The Committee's "Task Force on Physical Resources" held a special hearing on February 25, to hear Dr. James Fletcher, Administrator for the National Aeronautics and Space Administration, and Dr. H. Guyford Stever, Director of the National Science Foundation, specifically address questions affecting R&D budgeting. In addition, all other congressional committees, including the House Science and Technology Committee and the Joint Atomic Energy Committee, reported to the House Budget Committee on their legislative plans and spending priorities which included research and development.

April 15 is a key date in the congressional budget process. By this date, the House Budget Committee must send to the full House the "first concurrent resolution on the budget." This resolution reflects both the aggregate fiscal policy decisions and the relative spending priorities, divided among the sixteen budget functions, such as Defense, Agriculture, or Income Security. It should be noted that these functional categories overlap the jurisdiction of both federal agencies and congressional committees. These budget "targets" that Congress sets for itself were, I'm pleased to say, adopted prior to the statutory deadline of May 15.

Now, during the forthcoming months, Congress will run through its regular appropriations procedures. Shortly after Labor Day, the House Budget Committee will step back into the act. At that time, we are responsible for reexamining the state of the economy and the actual spending and revenue legislation that has been enacted by Congress during the summer. The Budget Committee must then recommend to the Congress a "second concurrent resolution on the budget," which the full Congress must adopt -- or reject -- by September 15. By law, the Congress cannot adjourn until it has agreed upon the second concurrent resolution. In this second resolution, Congress imposes upon itself a mandatory budget ceiling and sets a revenue floor for the fiscal year that, starting this year, will begin on October 1. It should be kept in mind that this second budget resolution may direct changes in spending or revenue legislation if it is not consistent with Congress' budget resolution.

It is hoped this second concurrent resolution, to be voted in September 1976, will complete the implementation of the new budget process. It will complete the passage from experimentation and development to actual implementation. In the first resolution we have established targets or goals for spending. The various authorizing committees and the Appropriations Committee can either abide by these targets or try to exceed them. There will be a struggle if there is an attempt to exceed the targets. If the targets have been exceeded, the Congress, by a reconciliation bill, can enforce its earlier decisions on spending or simply agree to whatever appropriations have been voted. This "reconciliation process" raises the question for the House Budget Committee and the House similar to the question in the old song, "Will you love me in September as you did in May?"

The Budget Process Impact on R&D Spending

Of immediate concern to you is how R&D fares in this process.

First, as you know, there is no such thing as an "R&D budget" per se either in the President's budget or in the congressional budget. The closest thing to an R&D budget can be found in one of the sixteen functional categories of the budget which is used to set spending targets -- it is category 250: "General Science, Space and Technology." This function includes the NSF, the non-aeronautical programs of NASA, certain research programs of the Energy Research and Development Administration (ERDA), and the science information exchange activities of the Smithsonian Institution. I can tell you that the current services budget estimate for this function was $4.56 billion, the President's request was for $4.5 billion, the House committees recommended in their March estimates an amount of $4.5 billion, and the first budget resolution adopted in May included a target of $4.5 billion.

Don't let the figures for this one budget category mislead you. R&D spending is scattered throughout the budget and appears in many other functional categories such as Health, Energy and Environment, Education, and Defense. The Office of Management and Budget (OMB) estimates that total R&D funding adds up to $23.5 billion, a sum five times larger than the budget resolution target in the General Science, Space and Technology function. The total funding target for R&D is not determined by a specific Budget Committee decision. Rather, it is the result of a number of separate budgetary decisions, some of which focus on parts of R&D spending and others of which focus on federal functions which include an R&D component.

With this picture of the congressional budget process in mind, the next question is: Over the next several years, will this process result in an increase or a decrease in federal research and development spending?

What Can Be Done to Affect R&D Spending?

Frankly, the answer depends in large part on you. As you well know, both inflation and the course of public events have taken their toll on R&D spending. Such events include the tightening of non-defense budgets during the Vietnam period, the conclusion of the Apollo program, a shift toward job-creating budget expenditures to combat the recession, and possibly a subtle disenchantment with science and technology which fails to quickly produce an easily identifiable product visible to the taxpayer.

In this context, one should bear in mind that federal support of research and development depends, fundamentally, on whether the public, the Congress, and the President are convinced that money spent on science and technology will buy something worth having. Research and development funding may be hurt by a communications gap between the scientific and engineering community on the one hand and the public policy-makers on the other. Research work has grown complex and remote, and there is a general lack of understanding on the part of the public and Congress of what research is and what it can produce.

For the general public, a review of the "Reports" in your widely read and excellent magazine Science could be a bewildering experience. Confronted with an article on "Cross-Modal Matching and the Primal Frontal Cortex," or on "Human Bloodstains: Individualization by Crossed Electro Immunodiffusion," a person might well turn to the sports page instead. I do not say this in a spirit of mockery. These are serious studies written, of necessity, in technical language. I know that every year you have to suffer the slings and arrows of some of my colleagues who have great fun with the more arcane titles of projects funded by the NSF, the Smithsonian, NASA, or the National Oceanic and Atmospheric Administration (NOAA). I am convinced that many of these projects have great potential value, even if they fail. The search for knowledge, which we call research, by definition cannot fail. Knowing something doesn't work is of scientific value. If there were no Lamarck, would there have been a Darwin? But it is one of the tasks of scientists, as it has been through history, to try to explain what they are doing, and to relate their work to everyday life.

Obviously, without a crystal ball scientists cannot say what products today's basic research will lead to ten years from now. But it can certainly be explained to an educated, interested public, who is without formal scientific training, what the broad relationships are between your research interests and the present technology that we know and take for granted. The public can be told what the future benefits of research might be, based on experiences in the past. After all, the jet plane, plastic wrap, the automobile, the long distance telephone, and the pocket calculator, now part of the accepted daily routine, are the result of countless hours and years of basic research. It is so easy to make sport of the "irrelevance" and "uselessness" of research projects with strange and incomprehensible names. I urge you to devote more effort to giving the public greater understanding of what you are doing and why it is important. I applaud this gathering as an important step in this direction.

I would also urge you to undertake a little public relations effort to highlight some of the economic impacts of R&D. Some recent studies performed by Chase Econometric Associates and Mathematica Incorporated suggest that federal R&D investment may have a highly leveraged effect upon the economy -- that is, a relatively large return can be gotten from a small investment. Specifically, the Chase Study, commissioned by NASA, found that every dollar invested in research and development yields a return of $14 to the gross national product over a ten-year period. This means increased employment and productivity. The Commerce Technical Advisory Board, in a recent report to the Secretary of Commerce titled "The Role of New Technical Enterprises in the U.S. Economy," found that between 1969 and 1974 five, innovative, high-technology companies with combined sales of $21 billion, created 106,000 new jobs. Another group of young, high-technology companies, with sales under a billion dollars, created 35,000 new jobs in the same period. At the same time, six mature companies with $36 billion in combined sales created only 25,000 new jobs.

The priority given to R&D by the Congress in its budget decisions will be influenced by the economic impact it has. Certainly these statistics are worthy of serious consideration when debating budget policies aimed at long-term permanent reductions in unemployment. It may be that while productivity improvements in traditional heavy manufacturing have generated a kind of built-in unemployment, high-technology companies that are labor intensive may hold the potential for productively employing available manpower.

Long-Term Budgeting Problems

In addition to your immediate concerns, there are two general and recurring budget problems which we must face.

The first is the question of controlling spending. Currently, nearly 70 percent of all budget expenditures are mandated under existing law. Neither Congress nor the President can do anything in any single budget to stop that spending. The proportion of budget outlays required by law has been rising each year. This is due in large part to increases in programs such as social security, unemployment insurance, and welfare which give individuals a legal right to payments. It is also due to long-term federal spending commitments that we have made to projects which stretch out over many years.

It could well be argued that the U.S. should make long-term commitments to the continuation of research so that research projects will not be buffeted annually by the changing winds of the budget process. The new AAAS budget project may lead you to this conclusion. However, we in Congress must be able to control expenditures at reasonable intervals if we are ever to meet our national economic goals. Let me assure you that the desire for guaranteed federal support and freedom from the worries of the budget process is probably the ultimate dream of every governor, mayor, government employee, businessman, and school superintendent. However, I firmly believe the treatment of most of the nation's expenses as inflexible long-term commitments dangerously reduces the options available for using the budget as a regulator of the economy, and can prevent the government from changing direction quickly enough to meet new problems.

The second recurring budget problem is that of impoundments -- the deliberate refusal by the Executive Branch to spend money appropriated by the Congress. Title X of the new Congressional Budget Act sets up a procedure whereby the President may temporarily or permanently withhold the expenditure of federal monies for specific programs, subject to review by the Congress.

While federal R&D spending has not been significantly affected to date by impoundments, I would contend that research spending is vulnerable because, frankly, it is a lot easier to turn off the budget spigot on a research project than it is to turn off the spigot on mandatory income assistance programs. Your programs may be vulnerable because of the long lead times between research spending and an identifiable product visible to the public. And, your programs will be vulnerable as long as research is perceived as a nicety rather than a necessity.

Conclusion

In closing, I would like to remind you that the United States has a tradition, dating back to our founding fathers, of federal support for technological innovation. Indeed, we should all recall that Article I, section 8, of the Constitution enumerates only seventeen specific powers of the Congress, and one of these seventeen is "To promote the Progress of Science and Useful Arts."

We must not drift away from our tradition of pride in the support of science. It is time for you as scientists to help us remember our tradition, to talk and to write about it, and to reestablish communication between the American public and the scientific community which has served our nation so well and contributed so much to our nation's prosperity and health throughout American history.

Thank you, Mr. Adams. That was one of the finest talks I've every heard. Let me make just one comment apropos of those remarks.

This morning I tried to attend all four workshops, and it was pretty maddening because I wanted to stay at each of them. But at the last one I visited, on long-range planning, I thought I heard some very optimistic remarks about the fate of the $50 million presidential increase for basic research in the National Science Foundation's budget. I don't believe we can take that increase for granted, by any means. It is vulnerable, partly for some of the reasons that Representative Adams has explained to us. The emphasis of the Budget Reform Act is on controllability. On the margin where things have to be put together and negotiated, that $50 million increase could be in considerable danger. As I understand it, the Senate Appropriations Subcommittee has come through and restored the House cut. The full Senate Committee has not acted, much less has the whole Senate acted, and the entire question is up for grabs when the bill returns to the House from the Senate. And anything can happen to it down there.*

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*Note Added in Proof: For a report on how the NSF budget fared, see: Walsh, John. "94th Congress: In an Election Year, Budget Process Makes an Impression," Science, 194, No. 4263 (October 22, 1976), 405-09.

The system that the Congress has set up is a very tight one. If it's going to work it cannot have very much leakage. Plainly, the more leakage, the more lack of credibility. So that's where we are. With all the talk about long-range planning to which I have been a very enthusiastic partner, we are facing a situation in which the political system is now very sensitive and oriented toward controllability. In that context we had better not be sentimental or theological about research and development, because those approaches are not going to have much impact.

Conclusion

Before I try to summarize my impressions of this colloquium, I think it worth suggesting that the active participation of both the AAAS President, William D. McElroy, and the Retiring President and Chairman of the Board, Margaret Mead, is symbolic of AAAS's interest in the whole field of science policy studies. We all had an opportunity to hear from Bill McElroy yesterday afternoon. Now I'd like to call on Margaret Mead for some impromptu remarks.

Margaret Mead

American Museum of
Natural History

I think we ought to recognize that the budget is a form of communication to the American people. When they look at the gross items of the budget, they draw their own conclusions about what the government thinks is important. Yesterday afternoon when the issue of training young scientists was discussed the assumption seemed to be that if we just had more money there would be more jobs, and that the reason more young people are not going into science is simply because of the shrinkage of academic and research possibilities.

Now, in 1956 Rhoda Metraux and I did a study of the attitude of high school students toward science.* There was no Vietnam, there was no Sputnik, there was no revived interest in science in this country. Yet, even in that year before Sputnik, the attitude toward federal participation in science and science participation in federal programs was beginning to harden. And the young people's picture was that there were two things wrong with science. First, it was devoted to war and destruction, and second, it introduced elements of veniality and corruption by too close associations between money and scientific activities. Yesterday Professor Doty noted that more students are now going into medicine and law because there aren't proper jobs available in science. But many go into medicine because they are interested in something related to life instead of death. They go into law because they are interested in what is happening to this country and they think that possibly law might be shaped into an instrument that would be of some use. They go into economics because they're impressed with the economic impacts of the energy crisis and believe that they ought to be thinking about it. When young people look at the federal budget and compare the amount put into defense with the amount put into other areas, they draw their own conclusions about the directions of federal policy.

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*Mead, Margaret and Metraux, Rhoda. "Image of the Scientist Among High School Students," Science, 126, No. 3270 (August 30, 1957), 384-90.

William D. Carey

Let me close this final session with a few personal impressions.

The Shapley report told us some things that are important and spelled out the details of a process that is becoming increasingly arcane. It also exploded some of the mythology that has traditionally enveloped that process.

We are better off, as a result. What now faces us is the problem of sustaining momentum as we go on from this point. I think it's clear that the AAAS has broken important ground in bringing government, industry, and academia into lively face-to-face discussions on a range of troublesome problems. We can't just walk away and leave it at that. There ought to be follow-up work done to keep this communication going, and I think it is up to AAAS to see to it. The first Shapley report must be followed next year by the second, perhaps with a different emphasis and treatment of budget and institutional questions. A second colloquium should occur in a year or so, keyed to analytic work that has been triggered by these two days of debate. Speaking both for myself and for the AAAS Committee on Science and Public Policy, I urge you to suggest how the AAAS should proceed in planning for the next round.

Thank you for coming and for contributing so much. Now we shall stand adjourned.

Appendix

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